THURSDAY, NOVEMBER 21, 1878

MATHEMATICS AT CAMBRIDGE.

THE Cambridge Examination for Mathematical Honours has for a long time enjoyed a high reputation, especially among Cambridge men, who have been accustomed to point to it as the model of what an examination should be. The credit thus claimed has been in past times more or less deserved, but to what extent it is so now is a question on which there may be variety of opinion. Like every other institution, the practical usefulness of which depends upon the ease with which it can adjust itself to external conditions, the Tripos examination must undergo changes to meet corresponding changes from the outside; and there may come a time when the external conditions operate so powerfully that mere modifications are insufficient, and when the changes made must be both radical and extensive. The old Tripos system has recently been put to a severe strain, and it is admitted on all hands that the result has proved unsatisfactory. We propose to inquire into the causes which have brought this about, and to discuss the measures by which it is hoped the evil will be met.

It may be laid down as a fundamental axiom that a university honours examination should be in harmony with the studies of the candidates, and that it should be reasonable in its demands upon them. What the very best students may be expected to answer after faithful work during their undergraduate course should clearly be taken as a guide in fixing a superior limit to the number and difficulty of the questions. Nor should this estimate be pitched too high, because there are other subjects of interest and study besides mathematics, in which it is desirable that even the best of young mathematicians should engage, and an education based on mathematics alone must necessarily be defective.

If we accept these propositions we must admit that the estimate of what is reasonable towards the candidates was very different fifty years ago to what it is now. One cannot help looking back with regretful eyes on the Tripos questions of that time, so remarkable for their simplicity and elegance, as well as for the happy appreciation of the degree of difficulty such questions should possess. The questions of more recent times, although they are often to be admired from an æsthetic point of view, are in many instances far beyond the reach of any but the very best men, and may be described as being somewhat too difficult and elaborate.

There are obvious reasons why, as time goes on, the questions should become more difficult; still it would be interesting to trace the changes over some considerable period, so as to be able to explain how the Tripos examination has reached its present form. The changes must have been of a gradual character, because the traditions and customs of the examination have been faithfully transmitted from one set of examiners to the next. One can see, however, that if any particular person were to examine often, as has occasionally happened, or if several persons of like tastes were to examine together, we should find the questions displaying a particular bias. When this should occur, the studies of the candidates would receive the same bias, and particular branches of mathematics would thus be pushed for a time into undue importance. So much was this the case about the year 1864 that Sir G. Airy, when delivering the Rede lecture in the Senate House, went out of his way to denounce the excessive attention given in the University to certain branches of pure mathematics.

We can thus imagine the Tripos examination based upon the traditions of its predecessors, yet continuing to grow both in extent and difficulty, and with some of its features perhaps somewhat exaggerated. In 1873 a great change had to be made. The University, feeling it was not creditable to it that so little encouragement should be given to the higher branches, and especially to the great modern subjects of mathematical physics, determined that those subjects should be introduced into the Tripos examination. The additions thus made, besides considerable extensions in the subjects already existing, included, amongst others, Elliptic Functions, Electricity, Magnetism, and Heat.

It was thought that in thus extending the examination the students would have a choice of subjects, and that their course of study would thereby be rendered more interesting than it was before. It was certainly never intended that the burdens under which the Tripos candidate was already staggering, should be increased upon him. But how were these changes met on the part of the students and on the part of the teachers? The immediate result seemed to be that the best candidates attempted to know something of all the subjects. The examiners in 1874 were careful to watch for indications as to whether the candidates had devoted themselves to special groups, but they reported that there was no evidence to that effect. The University thereupon passed a Grace enacting that the number of questions in the higher subjects should be increased, the object being to supply a sufficient variety so that a candidate who had confined his studies to a limited group might reasonably expect plenty to occupy him in the examination. This provision has continued operative down to the present time, but it does not appear to have produced the salutary effect intended. There is much reason to fear that the best candidates still push their way through most of the subjects, whilst the next best struggle as far in the same direction as they can.

There is then a well-defined evil to be remedied. For no one can deem it a good education where the student is carried, necessarily with rapidity, over a variety of subjects, many of which he must therefore very imperfectly comprehend. What changes in the examination are proposed to the University as a remedy we will presently describe. Meanwhile let us glance at the position of the teachers in their relation to the new state of affairs.

It must be admitted that the great changes made in 1873 found the college lecturers unprepared. There were only one or two of them who ventured to expound the new subjects to college classes. The students were therefore compelled to depend upon the private tutors, and thenceforth the selection of groups became difficult, if not impracticable. The fact we have just mentioned was, in truth, a misfortune in more ways than one. For the new subjects, and, indeed, the higher subjects generally

admit of being really well taught only by men specially devoted to them, which clearly the private tutors as a class could not very well be. The higher subjects must accordingly for some time be taught in an uncertain way, and meanwhile we are deprived of the evidence, which would have been useful in the present emergency, as to how the Cambridge system would work if these subjects were completely in the hands of the lecturers, as they should be.

As between college lecturers and private tutors we have some reason to hope that one effect of the introduction of the new subjects will be that the former will rise in importance and the latter will, at least relatively, decline. We wish to write nothing but good of the private tutors personally, but if that result should really take place we should regard it as a decided boon. There may be some cases in which private tuition may have merits of its own, but for the ordinary student to have a private tutor perpetually at his elbow when he meets with a difficulty is to give him the worst possible education. In like manner we do not admire the system of more than paternal supervision practised by many of the college tutors over their pupils, the natural effect of which is that the pupil is not allowed to act or to think for himself. He is perpetually asking and getting advice about very trifling matters, and receives a great amount of what is called individual attention on a variety of subjects. But though he may thus gain a little knowledge, if he ultimately learns habits of self-reliance he learns them from other sources.

If the system of private tuition could be done away with, and if more vigour could be instilled into the collegiate system of lecturing, so that complete and adequate courses of lectures could be given, there would be a healthier tone and spirit in the studies of the University, and we are convinced the students would learn more and learn better. In proof of this we may state that the most successful of the private tutors in mathematics really do what ought to be the work of the college lecturer; that is, they deliver lectures to their classes and examine written work for them. We may also state that in the department of classical studies most of the students depend solely upon the college lectures.

We have made these remarks because it seems to us that the Tripos examination is only one phase of the broader question of the whole system of mathematical teaching. Certain proposals will to-day be made to the University, and if these be carried the scheme of college lectures will have to be remodelled: if at the same time a new spirit and energy could be infused into them, it would be a good thing for Cambridge teaching.

The proposed alterations in the examination may be briefly described as follows:—The subjects are in the first place thrown into two grand divisions. On the one side ihere are what may be called the easier subjects, covertng all the ground which a moderately good candidate, whether his tastes incline him towards analysis or physics, may be expected to take up. On the other side there are the higher subjects of pure mathematics and physics. It is proposed that those two divisions should constitute the subjects of two distinct examinations.

The examination in the first division will, as regards class lists, take the place of the present Tripos, that is, the results will be given in the old form of Wranglers, Senior Optimes, and Junior Optimes, arranged in order of merit. This examination will take place in the June of the third year of residence, and only the Wranglers will be permitted to take up the second examination.

The subjects of the second examination which will take place in the following January are subdivided into four groups, and the results upon them will be given in classes, the names in each class being this time arranged in alphabetical order. It will be possible to attain a first class by doing well in one or two groups.

Those who bear unqualified hostility to competitive examinations, especially in their intensified form, when they are followed by an order of merit, will probably be satisfied for the present, hoping that at some future time they will succeed in abolishing the order of merit entirely. There are others who approve of the proposed changes, and who yet think that when confined within reasonable bounds such competitions can do no harm and may do good. Undergraduate human nature being as it is, a good contest, even such as can be had in a Tripos examination, is rather enjoyable than otherwise, and brings out qualities which are worth reckoning for something. It is also a good thing that an undergraduate should learn to have a piece of hard work well done in a given time. What has to be seen to is that the competition does not react injuriously on the course of study. There are many reasons which commend the proposed changes in that connection, and to one or two of these we will now advert.

In the first place, the higher subjects are not suitable for purposes of examination, because the questions which are likely to be put on them require long work and probably much reflection. It is good, therefore, not only that these subjects should be studied leisurely, but that the element of hurry should be as far as possible excluded from an examination upon them. It is true that the last remark will apply also to the lower subjects, but we are to consider that the latter subjects which usually consist of a few simple principles, admitting of an almost infinite variety of simple applications, are in a measure the tools of the mathematician who ought to be well skilled and expert in their use.

Again, if there is to be strict competition it is as well that the area should be narrowed and that the combatants should meet one another on common ground. In the present state of things that cannot be, but under the proposed system it will be possible for a clever lad who has read but little when he enters the University, to hold his own against a competitor, his inferior, who does not begin his undergraduate course till he has been pushed, or has plodded on a good way towards his mathematical degree.

The opponents of the proposed changes affirm that the Tripos will lose its prestige, and that the students under the new system will entirely neglect the second examination. It seems sufficient, in answer to the first of these objections, to point out that the first examination will not be so insignificant either in extent or difficulty, as not to compare in those respects with the Tripos examination of forty or fifty years ago. And as to the best students neglecting the second part, that is a circumstance which seems very unlikely to occur, but the colleges will have the matter in their own hands, and it is to be expected they would be patriotic enough to refuse their fellowships and certainly

their lectureships to students who had not distinguished themselves in both parts.

It is impossible to touch on all the points which suggest themselves in connection with this question, but we may point out in conclusion that the examination in Cambridge has to adapt itself to two classes of candidates, viz.: There is the class who may be called professed mathematicians, because they spend their lives in cultivating mathematical science and in teaching it to others, and there is the class who abandon their mathematics as soon as their undergraduate course has terminated. Of the former class we believe that their tastes and the necessities of their position will alike carry them beyond the subjects of the first examination. In the case of the second class, which is a large one, it is undoubtedly a wise thing to restrict their studies within the limits of the easier subjects. For under the present system, in their eagerness to secure good places they attempt subjects which are either beyond their powers or their opportunities, and so fail to gain the advantages which a strict mathematical training is supposed to afford.

"CRAM" BOOKS

Notes on Physiology, for the Use of Students Preparing for Examination. By Henry Ashby, M.B. (London: Longmans, Green and Co., 1878.)

THIS book, being a fairly creditable and careful specimen of its kind, seems to offer a fitting opportunity for denouncing the whole class of "cram" books of which it is a member. It purports to be notes on physiology, compiled originally, while the author was a demonstrator in the Liverpool School of Medicine, for the use of those students of the school who were preparing for the primary examination of the College of Surgeons; and it is confessedly based upon Foster's "Physiology" and the two chief anatomical text-books used in England. It is a small 18mo of about 230 pages, clearly printed in a large type, and it contains a number of condensed and dogmatic statements in all departments of physiology. It is, we rejoice to be able to say, written perspicuously and compiled with evident care. Most of what Mr. Ashby has read in Foster he has accurately digested and dogmatised. But though he has thus almost disarmed criticism as to his particular book, the book still remains infected with the vices of its class; it is a delusion and a snare to the student; and we heartily wish Mr. Ashby's talents had found a worthier object for their exercise. "Notes" are undoubtedly of the greatest value to a student—nay, they are indispensable, if he is to acquire a large view of his subject; but they are only valuable when the student has compiled them himself from the larger text-books, or, better still, from original memoirs, or when he has seen them digested and set down, so to speak, before his eyes by his teacher. Each of the sentences in his book Mr. Ashby doubtless could and would make the text of a lucid explanation in his lectures or demonstrations. He would lay before his hearers the different views of observers on different physiological questions, as he had learnt them, and, balancing the evidence, he would abstract for them a trustworthy judgment in a careful and concise statement: and the student who took down his notes, on re-reading them, would have the whole discussion refreshed in his mind with more or less vividness—would, in fact, have almost all the benefit of condensing the notes for himself. But when these concise statements or formulæ are put into the hands of students who have not been thus prepared for them, the case is wholly different. Aladdin has the lamp, indeed, but he can conjure up no powerful genii with it.

But if this were all we might be content to let books like this sink to their own level; their inutility would lead to their speedy death. But while the good student would never for a moment think of reading notes that he had not made himself, or if he did read those of another, would quickly find out the cause of their uselessness to any one but their author, the bad student is misled to believe that 230 small pages of fair-sized type contain the whole of the physiology that he needs; he looks through the list of contents and finds set down there almost every physiological fact and problem of which he has ever heard, and he naturally concludes that he has only to equip himself with this little book in order to cope with his examiner.

Mr. Ashby's book, admirable for the purposes of his own students, is useless or worse than useless to the students of any other teacher; published to the world, it is like a creature in an improper medium, and we are constrained to wish that, with all similar books, it may quickly meet the usual fate of creatures so circumstanced.

After this we need not say much about the book itself. On the whole it is well done. The histological sections are decidedly the weakest. The "ossification of bone" (p. 29), and the "development of tooth" (p. 107) might as well have been omitted altogether, as put in so meagrely. The extremely important histological researches of Heidenhain on the pancreas seem to be ignored on p. 16, where "probability" only is allowed to the elaborating functions of glandular epithelia. The pigment layer of the eye on p. 17 is assigned to the choroid coat instead of to the retina, and again on p. 194. No nucleus is given to striated muscular fibres on p. 81. On p. 179 Prof. Ferrier's name is put down at the end of a paragraph as if he were the prime authority for certain facts regarding the *corpora quadrigemina*, which we rather owe to Flourens, Longet, and Goltz. These errors are not of vital importance, and some of them have probably been due to inadvertence. But there are two more mistakes which are of greater weight, and show the danger of mere book-making. On p. 34, where the properties of muscle are discussed, we find that "On contraction . . . O is absorbed and CO_2 . . . given off." This is left unexplained here and in the rest of the book. What Mr. Ashby doubtless meant was that during contraction more arterial blood passes into muscle, and more O is taken up than during rest, while CO_2 is at the same time emitted; but, in the above unguarded way of statement, the fundamental fact of the independence of the actual absorption of O and disengagement of CO_2—a fact of the utmost moment in our conceptions of muscular work—would seem to be passed over. Again, on p. 168, under nervous conductivity, we have the curious statement that "the axis-cylinder probably conducts the impression, *the medullary sheath acting as a sort of insulator to prevent the currents from becoming mixed and confused*"—a physical explanation which no physiologist would now for a moment think of offering.

LETTERS TO THE EDITOR

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.

[The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.]

The Divisibility of the Electric Light

THE English and American periodicals devoted to electrical science now announce, "on authority," that the electric light discovered by Edison is a light by incandescence. If this be true there is nothing new or startling either in the discovery of the light or of its divisibility. Lighting by incandescence has been studied for a long time; indeed, it has been studied much more thoroughly than any other kind of electric lighting. Thirty-three years ago a method of producing and sub-dividing the light was patented in England by a Mr. King. The light was produced by heating to white heat in a vacuum, by means of the electric current, either platinum or carbons; and, the specification adds, "when the current is of sufficient intensity, two or a larger number of lights may be placed in the same circuit." For some years after this discovery several improvements on King's invention were patented in America, France, and England; "but," says M. Fontaine, "none of these appear more complete, more explicit, and more practicable than King's; it is, then, useless to continue our nomenclature." The principle of lighting by incandescence, although not neglected or forgotten, seems to have made but little progress until 1871, when M. Lodyguine showed an experiment in the Admiralty Dockyard in St. Petersburg, when he divided the circuit into no less than two hundred lights. This naturally made a great sensation at the time—as great a sensation as that caused by Mr. Edison's telegram of the 7th ult. The Academy of Science awarded to M. Lodyguine the large Lomonossow prize of 50,000 roubles. A company was formed in St. Petersburg with a capital of 200,000 roubles, and the excitement in Europe was then almost as great as has been witnessed in England lately. It was soon found, however, that Lodyguine's discoveries, like those of his predecessors in the same field were, after all, impracticable, and that his illimitable division of the light, however ingenious, was only a fanciful experiment. Every penny subscribed to the company referred to was lost, and Lodyguine's great discovery is now, where it was then—in his laboratory.

It has, however, been urged that these early inventors of the electric light knew only of the galvanic battery as a generator of a powerful current, and that had they known of the Gramme machine, or other dynamo- or magneto-electric machine, the results might have been different. The remark, however, only applies to King and the improvers who immediately succeeded him. The great division of the light by Lodyguine, to which reference has just been made, was in a circuit produced by two "Alliance" machines. Even, however, if such were not the case, there are at present before the world, in more or less detail, four recent inventions for the production of a divided light by incandescence. These are the inventions of M. Reynier, of M. Arnaud, of Mr. Edison, and most recent of all, M. Werdermann. From the way in which these discoveries—if they are discoveries—have been ushered into the world, it is found that great claims are made on their behalf, and there are, therefore, naturally great expectations on the part of the public in regard to them. It cannot be urged now in mitigation of the shortcomings of the incandescent light, as it has been urged in the past, that it has not had a fair trial, on the ground that the lamps in existence were imperfect in conception, and complex in construction. The lamp of M. Reynier seems admirable in its way, and if light by incandescence were to be the light of the future, the claims of this lamp would have to be very carefully considered, and, in any case, it will certainly hold an important place in all investigations into the subject. The lamp of M. Werdermann appears to be identical in principle with, and only slightly different in detail from, that of M. Reynier, and we may fully expect that these inventors will have to come to terms with each other—so much alike are their inventions. Of the details of Mr. Edison's invention—if there are any, nothing is known beyond the fact stated in the *Scientific American*, that it is a light produced from a spiral of incandescent platinum; while the reports in the American daily press show such an effervescent ignorance of the fundamental principles both of electricity and of dynamics, that no reliance whatever can be placed upon them.

Experience, then, has shown that a light by incandescence comes before us in a very questionable shape, and it is essentially a light which discourages the notion of its practical application. The question indeed may be very properly asked: How is it that light by incandescence has always proved such an utter failure? It has had a period of thirty-three years in which to develop; it has been divided into various lesser lights, numbering from two to two hundred: and it has arrested the attention and taxed the skill of the greatest electricians in the world. How is it that it is obliged to give way to light by the voltaic arc? The answer is at hand. The light by incandescence can only be obtained and divided by a great sacrifice of light and power. This is imperative from the fundamental principles of electrical science. The diminution according to the "square," and not according to simple proportion, applies to electricity just as it applies to light, heat, sound, gravitation, and other physical phenomena. Thus if a circuit be divided into two branches whose resistances are equal, a current of half the strength passes through each branch, producing at the point of resistance, not half the light, but only a quarter, because the effect follows the square of the current strength. If the current had been divided into three equal branches, in each branch only one-ninth part of the original light would be obtained, and so on; so that if an electric light of 1,000 candles were divided into ten equal lights, the result would be ten lights of ten candles each, instead of one of 1,000 candles. When this law is borne in mind, and when it is also remembered that to produce the electric light by incandescence at least one-half of the current is lost, it will easily be imagined what a wasteful light it is. Recent experiments prove this. It was recently stated, in reference to M. Werdermann's incandescent light, that he produced two lights of 320 candles each (total, 640 candles), with a prime mover of 2 horse-power; and this was considered a great result—as indeed it was for an incandescent light. But how this sinks into insignificance when compared with the results of lighting by the voltaic arc. A few days ago M. Rapieff, with two of his regulators and a small Gramme machine known as the M machine, and which M. Gramme says requires only 1½ horse-power, produced two lights, which, when carefully measured by the photometer, were found to be each equal to 1,150 candles, or a total of 2,300 candles, while with one of M. Gramme's A machines, requiring 2½ horse-power, a light of 6,000 candles can be obtained from one of M. Rapieff's regulators. Some experiments detailed in M. Fontaine's book on "Electric Lighting" gave a similar result. M. Fontaine's experiments with an incandescent light show that, under the most favourable circumstances, with a Bunsen battery of forty-eight cells, eight inches high, the diminution of the sub-divided light was so great that, where he put five lights in one circuit, he only obtained a total illuminating power of a quarter of a burner, with four lamps only three-quarters of a burner, with two lamps six-and-a-half burners, and with one lamp fifty-four burners. These numbers give the following ratio: 1, 3, 8, 26, 216, thus showing how rapidly the light diminishes when divided. With the voltaic arc, however, and with the same battery, he was able, by a Serrin lamp, to obtain a light of 105 burners.

It will be seen, then, from what has been above stated, that the production and the divisibility of the light by incandescence is a very wasteful process—so wasteful, indeed, as to render its practical application impossible for general lighting. If, therefore, all Mr. Edison has to announce to the world is that he has succeeded in dividing an incandescent light—and the announcement that such is so is made on authority—his discovery amounts to very little. Both the light and its divisibility were discovered long ago. It will easily be seen that it is not in that direction that any great practical results can be obtained. The voltaic arc supplies the only divisible light of any utility and economy, and it is in its development that any real progress must be looked for.

WILLIAM TRANT

Duplexing the Atlantic Cable

I HAVE read with surprise in your number of the 14th inst. (vol. xix. p. 38), an article, in which it is implied that the application of the duplex method of signalling to an Atlantic cable has now for the first time been successfully accomplished by Mr. Stearns.

The publication in the *Times* of Sir James Anderson's letter on "the duplex system in telegraphing," on the day after the publication of your article, was a coincidence of which I trust

you will in fairness allow me to take advantage, to prove that your article does scant justice to Mr. Stearns' predecessors in the application of the duplex system to long submarine cables, and that their success has been something more than "only partial" in the opinion of those who have employed their system.

Mr. Stearns' first success on a long cable dates from a few days ago. In February, 1876, Dr. Muirhead and myself obtained experimentally a perfect balance on the Suez-Aden cable, which, though shorter in miles, is electrically longer than either of the Anglo Company's cables from Valentia on which Mr. Stearns has worked.

In March of the same year Mr. J. Muirhead and myself duplexed the Marseilles-Malta cable, which, though only 825 miles in length, is worked by Sir W. Thomson's syphon recorder, and our system has been in commercial operation on the line ever since.

Early in 1877 Dr. Muirhead applied the system to the Aden-Bombay Cable, which is longer in miles and far longer electrically than either of the cables from Valentia, and since that time this line, as well as that from Suez to Aden, has been worked "duplex" whenever the traffic required it, to the entire satisfaction of the company.

Next, as to your remark that "Mr. Muirhead has been at work duplexing the Direct United States Cable with some prospect of success," the facts of the case are these:—

The cable, in its linear measurement, exceeds the longest Valentia cable by 543 miles; electrically it is twice as long.

It is worked with the mirror galvanometer, and not with the recorder, and these circumstances render the difficulty of obtaining a duplex balance upon it immensely greater than upon any of the other lines referred to.

Notwithstanding the difficulties mentioned, Dr. Muirhead and myself, in April last, obtained a perfectly satisfactory balance, enabling us to transmit sixteen words a minute in both directions at the same time, between Ireland and Nova Scotia, a cable distance of 2,420 nautical miles. HERBERT TAYLOR

7, Pope's Head Alley, Lombard Street

P.S.—Since writing the above my attention has been called to NATURE, vol. xv. p. 180, containing an article on this subject, in which the applications of Muirhead's system to some of the cables referred to in my letter are spoken of as being the first practical successes in submarine duplex telegraphy.

Remarkable Colour-Variation in Lizards

MR. WALLACE's observations in NATURE, vol. xix. p. 4, on a black variety of the common lizard of Capri, as met with on the neighbouring islet of Faraglioni, induces me to refer to a similar appearance in the lizards frequenting the islet of Filfla, on the southern coast of Malta. As recorded in my book, "Notes of a Naturalist in the Nile Valley and Malta," p. 80, I have stated that during a visit to Filfla I was surprised to find that all the lizards on the rock were a beautiful bronze black and so much tamer than their timider brethren on the mainland. Many individuals were so tame that they scrambled about our feet and fed on the refuse of our luncheon. I subsequently sent specimens of this variety, or rather race, to Dr. Günther, F.R.S., who pronounced them identical with the *Podarcis muralis*, so extremely plentiful in Malta and Gozo. Now although the denizens of the two latter islands present divers shades of colouring, I never observed (and I looked carefully during several years) a black or dark-coloured individual. Filfla is about 600 yards in circumference and three miles distant from Malta. It is formed of the upper miocene limestone, and marks an important fault or down-throw which runs along the coast of Malta opposite, by which, as seen in the sketches Figs. 1 and 2 of the work referred to, it appears clear that the severance took place long subsequent to the days of the pigmy elephants, hippos, giant dormice and tortoises, whose remains have been found in such abundance in the crevices of the rocks opposite Filfla. There is no verdure on this bare rock-islet, the surface of which is dark-coloured, whilst its crevices shelter the lizards and furnish abodes for the nests of Manx and cenereous shearwaters, whose docility at the breeding season is equally remarkable, both reptile and birds being like their compeers of Enoch Arden's island, "so wild that they were tame."

Probably the dark colouring is protective, and thus consorting well with the surrounding surfaces, would tend to preserve them from the harriers, buzzards, and hawks which tarry in the Maltese Islands during the spring and autumn migrations

November 11 A. LEITH ADAMS

THE remarkable case of local colour-variation in lizards communicated by Mr. A. R. Wallace to NATURE (vol. xix. p. 4), had already been investigated by Dr. Theodor Eimer, an abstract or translation of whose memoir on the subject, entitled "*Lacerta muralis cærulea*, a Contribution to the Darwinian Theory," is to be found in *Ann. and Mag. Nat. Hist.*, 1875, 4th ser., vol. xvi. p. 234. J. WOOD-MASON

54, Claverton Street, S.W., November 16

The Drought

AT the present time, when more attention is paid to the influence of meteorological phenomena upon society, it would be useful to give some information as to the bearing of the local droughts and famines on our trade and the prospect of its revival. The China and Indian trades have not yet recovered. The droughts have also affected Egypt and Morocco. In the West Indies, Guiana, Venezuela, Colombia, and Brazil they are still operative.

They act to prevent the growth of produce, and in many countries, by reducing the water-ways, they impede its shipment. The people cannot consume our imports, the transit of which is in some cases impeded. The whole of these difficulties affects the exchanges and interferes with the money market and remittances.

The severity of the crisis is abating, but we can hardly feel assured of the revival of trade in Europe and the United States till there is a complete recovery over the vast areas of producing and consuming countries.

Thus the study of meteorological phenomena and facts acquires a new value for practical men and society at large, as stated by Prof. Jevons in your last number. HYDE CLARKE

Sewerage and Drainage

IN NATURE, vol. xix. p. 1, you touch upon a most important point in sanitary engineering which I have for ten years been striving by every means in my power to press upon the public, and I therefore venture to trouble you with a few lines on the subject.

The most important argument in favour of the exclusion of storm water from sewers consists, as you say, in the liability of road detritus to form deposits on the wide flat surface of any channels large enough to convey to one point an exceptionally heavy fall of rain over the area covered by a town, and the inevitably slow course of the infinitely smaller volume of sewage flowing or stagnating in dry weather along the same channels.

When separate sewers are provided for sewage they can be made of such smaller capacity as to keep up a constant flow from the houses in which the sewage is produced, to the land upon which it is to be purified, because the volume of liquid will very nearly correspond with the water supply, and the engineer has safe data upon which to adjust his means to the desired end.

In every town there are, or were, lines of natural watercourses, and if the scavengers' work is properly done the rain-water from roofs and streets may safely be discharged into any of these by short lengths of drains, less liable to be encumbered with deposits of road detritus, and with the certainty that if such accumulations should occur, they will be perfectly harmless from the absence of sewage.

The experiments of Mr. Way with London street water have been seized upon by Mr. Baldwin Latham in order to cover his retreat from the false position unfortunately taken up by himself and most of our senior engineers in the earlier days of sanitary science, and as he knows as well as any one else that it was a grand mistake to confuse and combine *sewerage* and *drainage* in one system, I agree with you in thinking it a pity that he has not acknowledged the facts more distinctly in the recent edition of his well-known work.

The greater proportion of the impurities detected by Prof. Way in the few samples of London street water which he tested are mineral ones which would be comparatively harmless, and, in the opinion of Dr. Voelcker, the experiments must have been vitiated by some mistake. Now as the latter authority has

publicly stated this and added his opinion that no sanitary authority could possibly object to water from streets and roofs of houses, uncontaminated by sewage, being passed directly into any river, Mr. Baldwin Latham might have been expected to have investigated the subject further before adducing the one statement without the other.

There are many other arguments of a sanitary and economical nature in favour of the collection of sewage without more dilution than necessary with rain water, but I must not trespass further on your valuable space.

ALFRED S. JONES

Rayons du Crépuscule

MY father tells me that he sees this phenomenon about five times in the year on the average in this climate. But the display at 4.40 this afternoon was unusually brilliant. The morning had been very wet, and when it cleared in W.N.W. at 2.30 we noticed the clear sky to be of an unusually green tint.

Very distant cirro-stratus on the south horizon ceased to be illumined by the rosy sunlight about twelve minutes before the phenomenon became visible. The latter consisted of very bright rosy rays, in a *very clear* sky, converging near the E.N.E. horizon, the moon shining very brightly on the left of the place of apparent convergence. The sky in interspaces between these rays was of a deep blue; these interspaces being, I suppose, the shadows thrown from distant cumuli and shower-clouds, some of which could be seen upon the western horizon.

Ashby Parva, Lutterworth, November 10 ANNIE LEY

The Power of Stupefying Spiders Possessed by Wasps

WITH reference to my letter on this subject (NATURE, vol. xviii. p. 695), Mr. Arthur Nicols writes me his disbelief that the pretended taking of the poison of snakes internally as a supposed antidote or prophylactic against the bite is anything more than a juggle of those chartered charlatans, the snake-charmers of India; or that it can be so taken with impunity. Of the wasps he says: "I dare say you know that one of the mason wasps of Australia glues its egg to the inside wall of the mud nest, and always at the top, while the rest of the space is filled with spiders. The sting of this wasp is a terrible affair. I was rendered quite comatose for several hours by being stung in the knee" [which, by the way, is precisely the condition of the spiders], "and the pain was most excruciating, with aching and swelling of the inguinal and axillary glands. I don't know whether the wasp stings the spiders, but they are always in a good state of preservation, even when the egg is on the point of hatching. I never found one in the least decomposed. The nest, however, is hermetically sealed, and decomposition could hardly take place, because so very small a quantity of oxygen is inclosed within." It will be remembered that Mr. Armit remarked "a constant movement in the legs of the spiders," and that observation has been made before.

If the word "wasps" in the above heading (which is not mine) be understood to refer to any of the true vespæ (*vulgaris, rufa, britannica*, or *borealis*), I agree with Mr. Frederick Smith (NATURE, vol. xix. p. 32), that it is misleading. But the solitary insects whose habits have been referred to, have been called wasps in all the popular books of natural history with which I am acquainted; and the correctness, or incorrectness, of the English name does not affect the points brought forward. I may, perhaps, be allowed to recall that the one object for which I referred to the Athenian insect—in connection with a correspondence then going on in your columns as to the senses of insects—was the remarkable circumstance that it seems to hunt down its prey *by scent*. Mr. Armit, of Queensland, referring to that letter, asks the further question, How are the spiders stupefied, and not killed, by a sting, formidable enough in one species to endanger the life of a man? Mr. Smith's letter throws no light on either of these points.

Bregner Bournemouth HENRY CECIL

The Ayrshire Crannog

THE remarks made by Dr. Buchanan White in your last issue in regard to the supposed existence of beech and the absence of Scotch-fir in the Ayrshire Crannog will be carefully attended to. Birch and hazel, so easily recognised by the bark, are certainly in greater abundance than any other kind of wood. I shall, however, collect as many specimens as I can find and submit them to the examination of competent authorities and publish the result in due course. We have now made a large addition to our list of relics, among which I may mention the following:—three daggers (one of which has a gold band round the handle); one knife, one gouge—all these are made of metal, of which the gouge alone has been tested and found to be bronze; a polished stone celt; a clay spindle whorl partially perforated; a curious fringe-like object made of vegetable material; several implements of bone and deers'-horn; a piece of wood with carving on it; portions of a flat dish cut out of wood; a wooden scraper cut out of a trunk of a tree with the handle formed of a branch growing straight out from it—(beside this scrape about a handful of short black hair was found);—a double paddle of a canoe together with various other wooden implements. Hitherto not a single fragment of any kind of pottery has been found on the Crannog. Being merely an amateur in this kind of research, I shall be glad to receive any suggestions from experienced gentlemen as to important points that should be looked after. ROBERT MUNRO

Kilmarnock, November 16

ON THE UTILISATION OF THE AFRICAN ELEPHANT

THE *Colonies and India*, of November 2, contains a short but suggestive article under the heading "Notes," "Elephants in Cape Colony," which deserves consideration. It states that elephants are numerous in the interior of Cape Colony as well as in Central Africa, yet no one seems to have attempted to catch and tame them. The subject has already been mooted that there is a good field for their use both in Central Africa and in Cape Colony, and that they would prove a new and important method of opening up and utilising the wealth of the Colony and of furthering the explorations in Central Africa, which are now of such general interest.

It appears that a troop of wild elephants has been observed within fifty miles of Port Elizabeth—on these the attempt might first be made—and it is well known that they abound in Central Africa, where, indiscriminately slaughtered for the sake of their ivory, the destruction of these animals is so great, as at no very distant period to threaten their extinction. It seems worthy of consideration whether it would not be better to attempt to utilise them as beasts of burden, as is done in India, where they are of inestimable service to the Commissariat, the Public Works Department, the planters, and many others. The African differs from the Asiatic elephant in some points, but is equally well adapted for labour, and, there can be no doubt, would be as easily tamed and trained as his Indian congener. That this is the case is amply proved by the docile and submissive state into which the male and female African elephants now in the Regent's Park Gardens have been brought by Mr. Bartlett and their keeper, Scott. They appear to be just as obedient, intelligent, and free from vice as Indian elephants, and there is, I think, little doubt that the one species, under proper training and discipline, would be as useful in Africa as the other is in India.

There is every reason now to hope that the wealth and resources of our South African possessions will undergo development—might it not be well to revive the suggestion that the elephant should be enlisted in the good work? The importation of one or more of the numerous officers who have been trained to the work of catching and domesticating wild elephants in India with a fitting establishment, and, perhaps, a few Indian elephants to commence the work, would very soon put the value of the undertaking to the test, and probably show that a vast source of working power now unused might be made available.

It is probable that in ancient times the African elephant was domesticated, and any one who has studied the two magnificent specimens in the Society's collection in

Regent's Park, will, I think, be satisfied that they may again be so, and that in temper, docility, and working power, they would be equal, if not superior, to the Indian elephant.

Through the medium of the columns of NATURE, perhaps an impetus may be given to a matter that is certainly worthy of consideration, and may elicit further remarks from some of the Indian Keddah officers, who are practically experienced in the mode of dealing with elephants. It is, at all events, worthy of Sir Bartle Frere's consideration.

J. FAYRER

SYNCHRONISED CLOCKS

RAILWAYS, among many other services which they have rendered, have made us more particular about keeping our watches and clocks in accordance with some common standard of time, and during the past few years various systems have been tried for the distribution of a standard time from a common centre—in this country Greenwich Observatory. For purposes of public life it is more important to have all the public and even private time-pieces of a country set according to one standard, than that they should show the correct local time. The latter can easily be ascertained by any one who desires it, if he can be sure of knowing the exact Greenwich time. Of all the systems that have been tried for ordinary public use, that recently organised by Messrs. Barraud and Lund, of Cornhill, seems to us to answer all the requisite conditions. We had the pleasure the other day of inspecting the arrangements made by Messrs. Barraud and Lund for the distribution of Greenwich time from Cornhill as a centre, and we are bound to say that the perseverance and ingenuity displayed deserve success, and we believe that wonderful success has been obtained. Messrs. Barraud and Lund have spared no pains and no expense to perfect their system, which, as a practical and widely useful application of science, is full of instruction.

Any system for the public service of time-signals by synchronising currents which lays claim to approximate perfection, naturally divides itself into three distinct departments; 1. The maintenance of a standard time-keeper at absolutely correct Greenwich mean time; 2. The distribution of the time at hourly intervals with the needful apparatus for testing the work done; and last but not least, the particular means adopted to enable the time currents to control or set the various clocks in circuit to true time. As to the standard adopted by Messrs. Barraud and Lund, and which is in direct communication with Greenwich, this is a mercurial regulator of the very best construction with a Graham dead-beat escapement, having the contact springs for the time-current between the dial and upper plate, the dial being pierced so as to allow free access to all parts of this mechanism without otherwise stopping or interfering with the regulator. However excellent such a time-keeper may be some error will always exist; it will have a daily rate however small, and it becomes important that this error should be corrected at least once a-day. In order that this may be effected without actually using any physical connection, Messrs. Barraud and Lund have adopted the following method:—A permanent bar-magnet about 6 inches long, is secured to the pendulum-rod, so as to vibrate about $\frac{1}{32}$ of an inch from a resistance-coil fastened to the case by a projecting bracket; a current of electricity passing through this coil, accelerates or retards the vibrations of the pendulum according as the current sent is positive or negative, and the power of the current is adjusted to give one second of influence for one hour of duration. The wires of this adjusting current are connected with a commutator, a small instrument about two inches by three-and-a-half, having three holes—that in which a plug is normally placed when no effect is required to be produced, a second marked "fast," and a third "slow," for the reception of the plug, according as the standard requires to be accelerated or retarded. In order to secure the continuance of the current for a period neither longer nor shorter than will produce the desired result, a small ordinary clock is interposed between the commutator and

FIG. 1.

the standard clock. This clock answers a double purpose; it has no hour hand, but only a minute hand, which stands normally at twelve, in which position the clock is "stopped," and no current can pass through to the resistance coil. The plug having first been placed in the "fast" or "slow" hole, as occasion may require, the

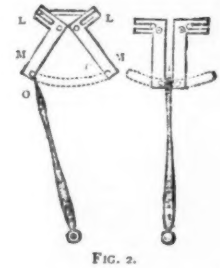

FIG. 2.

clock is set back, say one half-hour for half a second error of the standard. The mere fact of setting back this hand, *starts the small clock and closes the current* at the same moment, continuing to keep a closed current, till the hand returns to 12, when it again breaks contact and stops itself. It will thus be seen that one operation alone

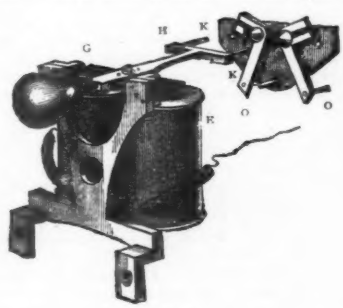

FIG. 3.

is required to set the standard, the whole action automatically ceasing the moment the standard is "to time."

The "distributor" is, in fact, a compound "relay" for twelve circuits, it having been determined to use independent batteries for each line wire in preference to split currents, as used in the chronopher at St. Martin's-le-

Grand. The appearance of this "distributor" is very much like a miniature cart-wheel, about five inches in diameter, inclosed in a circular glass-case, the spokes being the twelve contact springs and the nave a small insulated table which, when the standard automatically closes the contact springs of the time-wire at the exact sixtieth second every hour, is instantly carried down upon and closes the twelve pair of contact-springs connecting the twelve line-wires with their batteries.

Many interesting methods have been adopted for testing and other purposes of maintenance, but we have only space to mention two. One is for the purpose of warning in case of a break-down. This consists of an ordinary electric clattering bell, with a small clock attached, and also connected with the line-wire. So long as the time-currents pass regularly through this instrument the alarm-bell remains silent; but five minutes after the failure of any time signal to pass out upon its duty, the alarm-bell begins to ring violently, continuing to do so for five minutes, and then leaving a small drop indicator, showing on which line the break-down has occurred. There is only one alarm-bell, but each line requires its attendant little timepiece (which, being "synchronised" by the passing current, never can be wrong) and drop indicator. The other instrument is an alarum answering the following important purpose:—A break-down being reported, a plug is placed in a "bridge" corresponding to the number of the line, which causes a very weak current (too weak to affect any of the synchronisers) to pass out along the line for the use of the linesman when searching for the fault; this found and repaired it is important that the plug should be immediately removed: to call attention at the office in Cornhill the linesman has only to break contact at the nearest clock anywhere on the line to set the alarum going in the instrument room, when, the plug being removed, the alarum is shunted off and the line clear for work.

We now come to the chief specialty of the system of Messrs. Barraud and Lund, namely, the synchroniser. This is an automatic finger-and-thumb action, brought to bear hourly on the minute-hand, and bringing it, whether fast or slow, "to time." Each synchroniser is complete in itself, and is simply screwed behind the dial of the clock to be synchronised. It is as follows:—

Two levers carrying pins, and representing the human finger and thumb, project through a slot in the dial (Fig. 1), and either close upon the end of the minute hand itself, or upon a small block fastened to it underneath; the two levers have at their other end slots in which two pins work, projecting from the keeper of an electro-magnet, which, when magnetised at the given moment by the closing of the contact springs on the standard clock, draws the pins together, and sets the hand to time. A reference to Figs. 2 and 3 will at once make the *modus operandi* clear. E is an ordinary electro-magnet, G the keeper, carrying a bar, H, and two projecting pins, K K; these act in slots L L in the levers M M, from the ends of which project the pins O O (representing the human finger and thumb); the passage of the current draws down the keeper and bar, acts upon the levers M M, closing them upon the minute hand of the clock and setting it, whether fast or slow, to time.

The following are some of the special advantages claimed for this system:—1. That any number of clocks, few or many, of any varying sizes, can be synchronised to any agreed standard time-keeper. 2. That the mechanism is, when not in its momentary use, entirely detached from the works of the clock. 3. That it can be applied to existing clocks by simply being screwed in its place, and a narrow slit cut in the dial. 4. That any failure in the transmission of the time-current leaves the clock going in the ordinary way, to be "*set to time*" by the next completed current. 5. That the clocks are kept to time, whether having otherwise either a gaining or losing rate, even if such rate amounts to many minutes a day.

It will seen from the above description that the system owes its success not to the discovery or application of any new fact, such as that for which such eager search is now being made to secure a perfect electric light, but from the simplicity and efficiency of the synchronisers, and their adaptability to every kind and size of clock. By the simple expedient of winding the coils of all the instruments with one size wire, any number, all of varying sizes and powers, can be connected up in circuit. In the City circuit alone, which is wholly controlled by the standard at 41, Cornhill, 108 clocks on eleven different lines of an aggregate length of ten miles, and connecting over eighty different establishments, are efficiently kept to true time. Many more, we believe, are kept to time, not only in other widely distant parts of London, but in various parts of the country; for the latter purpose Messrs. Barraud and Lund have a contract with the Postal Telegraph Department for the delivery of time currents at certain hours each day. There is now no reason why all our public clocks at least should not be included within the correcting power of this system, and lead us astray no longer.

THE TELEPHONE, ITS HISTORY AND ITS RECENT IMPROVEMENTS[1]

III.

The Carbon Telephone

IN the columns of this journal (NATURE, vol. xvii. p. 512) the present writer remarked in the early part of the year "that it was unlikely the telephone of the future would employ the voice to generate the driving power, as it does in the magneto-telephone, but only to modulate the flow of a current obtained by coarser means. It is in this direction that Mr. Edison is working, and his practical triumphs in the past are the earnest of success to those excellent telephonic investigations wherein he has already won an enduring fame." Since those words were written Mr. Edison has brought his telephonic experiments to so successful an issue that his carbon transmitter and his new receiver leave little to be desired in electric telephony, except the automatic record of the received speech, and this, it is not impossible, may ere long be accomplished.

The object Mr. Edison sought to attain was a variation in the resistance of the circuit proportional to the motions of the vibrating diaphragm of the transmitter. Gray employed for this purpose a liquid resistance, but owing to the fact that all suitable liquids are decomposed by the current, Edison abandoned them and tried solid conductors. He remarks in Prescott's work on the telephone:—

"In the spring of 1876, and during the ensuing summer, I endeavoured to utilise the great resistance of thin films of plumbago and white Arkansas oil-stone, on ground glass, and it was here that I first succeeded in conveying over wires many articulated sentences."

A spring attached to the vibrating diaphragm was arranged so as to cut in and out of the circuit more or less of the plumbago film. But the results were not very favourable. In January, 1877, a new device occurred to Edison, namely, the employment of a peculiar property which semi-conductors have, of varying their resistance under pressure.[1] For this purpose

[1] Continued from p. 14.

[2] We have already considered in a previous article the historical facts connected with this discovery, and therefore it will be needless to refer to this point here. A reference to the *Journal Télégraphique* of Berne for 1874, wherein it was asserted M. Clérac had anticipated the use Mr. Edison has made of the varying resistance of carbon dust under varying pressures, fully confirms the statement we made in our last article that the merit of this application is not due to M. Clérac at all, who simply used *permanently compressed carbon dust* as a rheostat.

the diaphragm was made to press against a little cylinder of crude plumbago. The articulation was poor, though conversation could be understood. Investigation showed that the difference of resistance produced by varying pressure was exceedingly small. As so small a change in a circuit of large resistance was but a small factor, whereas a slight change of resistance in the primary circuit of an induction-coil would be an important factor, it occurred to Edison to associate an induction-coil with his arrangement. But difficulties arose from the high resistance of the plumbago cylinder he first used. Ultimately he constructed a transmitter in which a thin slice or button of a semi-conducting substance was placed between two platinum discs. Electrical connection between the button and discs was maintained by the slight pressure of a piece of rubber tubing which was secured to the diaphragm, and also made to rest against the outside disc. The vibrations of the diaphragm were thus able to produce the requisite variations of pressure on the button and thereby create corresponding variations in the resistance in the primary circuit of the induction coil; which in turn gave rise to a corresponding series of induced currents in the secondary. Finally, these induced currents were transmitted through the line and received at the far end by an ordinary magneto-telephone. Fig. 1, for which we are indebted to the *Telegraphic Journal*, shows the arrangement.

At first a button of solid plumbago, such as is employed by electrotypers, was used, and the results obtained were considered excellent, everything transmitted coming out moderately distinct, but the volume of sound was no greater than in that of the magneto-telephone. Numerous other semi-conductors were tried until Edison hit upon some lamp-black that had been taken from a smoking petroleum lamp and preserved as a curiosity on account of its intense black colour. A small disc was made of this substance, and when placed in the telephone it gave splendid results, the articulation being distinct, and the volume of sound several times greater than with a pair of telephones worked on the magneto principle. It was soon found upon investigation that the resistance of a disc formed of this substance could be varied from 300 ohms to the fractional part of a single ohm by pressure alone,[1] and that the best results were obtained when the resistance of the primary coil, in which the carbon disc was included, was six-tenths of an ohm, and the normal resistance of the disc itself three ohms.

The rubber tube between the diaphragm and the disc gave some trouble on account of its tendency to become flattened. Experiments undertaken with a view to remedy this defect led Edison to discover that not only could a rigid substance be interposed with advantage, but that the vibrating diaphragm even was unnecessary; that, in fact, the sound-waves could be transformed into electrical pulsations without the movement of any intervening mechanism. Edison states that the manner in which he arrived at this result was as follows:—

"I first substituted a spiral spring of about a quarter-inch in length, containing four turns of wire, for the rubber tube which connected the diaphragm with the discs. I found, however, that this spring gave out a musical tone which interfered somewhat with the effects produced by the voice; but, in the hope of overcoming this defect, I kept on substituting spiral springs of thicker wire, and as I did so I found that the articulation became both clearer and louder. At last I substituted a solid substance for the springs that had gradually been made more and more inelastic, and then I obtained very marked improvements in the results. It then occurred to me that the whole question was one of pressure only, and that it was not necessary that the diaphragm should vibrate at all. I consequently put in a heavy diaphragm, one-and-three-quarter inch in diameter and one-sixteenth inch thick, and fastened the carbon disc and plate tightly together, so that the latter showed no vibration with the loudest tones. Upon testing it I found my surmises verified: the articulation was perfect, and the volume of sound so great that conversation carried on in a whisper three feet from the telephone was clearly heard and understood at the other end of the line."

The present and modified form of the instrument is shown in the next diagram, where A A is the thick iron diaphragm, B the rigid connecting-piece pressing together the metal discs D D and the carbon disc C. The pressure can be regulated by the screw S acting upon the sliding stem E, which terminates in an insulating cup that encloses the carbon and metal discs. Wires lead from D D to binding screws.

It has been urged that Edison was led to adopt this arrangement by the discovery of the microphone, it is therefore of historical interest to note that in the *American Journal of the Telegraph* for April 16, 1878, it is stated "In the latest form of transmitter which Mr. Edison has introduced, the vibrating diaphragm is done away with altogether." A week or two later the discovery of the microphone was announced, and the transmission of speech without a vibrating diaphragm aroused universal surprise; the loose contacts which are essential to the microphone are, however, fatal for telephonic purposes.

It must be understood that the carbon telephone only acts as a transmitter; it is incapable of reconverting the electric pulsations into sonorous vibrations. For this purpose the ordinary magneto-telephone is employed.[1] The accessories and electrical connections requisite for use in a carbon telephone circuit are shown in the next diagram (Fig. 3).

P P is the primary wire of an induction coil having a resistance of several ohms and placed outside instead of, as is usual, inside the secondary coil *s*, which has a resistance of some 200 ohms.[2] R is the receiving magneto-telephone and T the transmitting carbon telephone; either one or the other can be thrown into the circuit by means of the switch K. When a plug is inserted at the bottom of the switch between 3 and 4, the relay or sounder S, battery B, and key in the centre of the figure, are included in the main line circuit. This is the normal arrangement of the apparatus for signalling purposes. To call the distant end the key is pressed down two or three times; by this means battery currents are sent through the primary coil P, the currents thus induced in the secondary coil *s*, pass to line, and actuate the relay or sounder in the distant instrument. When a plug is inserted at the top, between 1, 2, and 4, the apparatus is available for telephonic communication. By tracing out the connections it will be seen that in this latter case the battery, B, the primary wire of the coil, P, and the transmitter, T, are in short circuit, and at the same time the line wire is in circuit with the secondary coil, *s*. A general view of the arrangement is shown in Fig. 3, for which we are indebted to the publishers of Count du Moncel's book on the telephone. The lettering is different, but the respective parts can be readily understood. In this case a polarised relay and electric call-bell are employed instead of the sounder, a necessary addition in long circuits.

Concerning the actual performance of the carbon tele-

[1] The present writer can confirm this statement, but everything depends on the exact quality of the lampblack, the least trace of overheating lessens its intense blackness and enormously diminishes its conductivity.

[1] The use of the Bell telephone as a receiver in Edison's instrument is at present the subject of legal proceedings; Edison, however, claims to have used the magneto-receiver before Bell invented it, and we learn that a letter has recently arrived from Mr. Edison stating that he has now constructed a still better and novel receiver for his telephone. Edison remarks: "Batchelor, one of my assistants, heard a whisper last night fifteen feet away from the receiver, and ordinary conversation comes out as loud as originally spoken." Further information about this receiver is given at the close of this article.

[2] In the last improvements the usual position of the primary and secondary coils has been reverted to; the resistance of the former for short circuits should be about a third of an ohm and of the latter somewhat over seventy ohms.

phone, it is stated in Prescott's work on the telephone that Mr. Bentley, President of the local telegraph company at Philadelphia, has succeeded in working with it over a wire of 720 miles in length, and has found it a

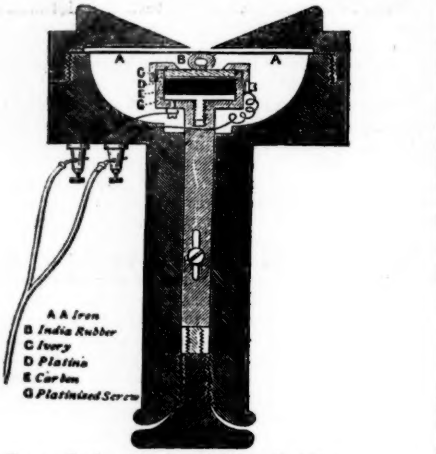

FIG. 1.—Section of Carbon Telephone—Early form.

practicable instrument upon wires of 100 to 200 miles in length, notwithstanding the fact that the latter were placed upon poles with numerous other wires, which occasioned sufficiently powerful induced currents in them

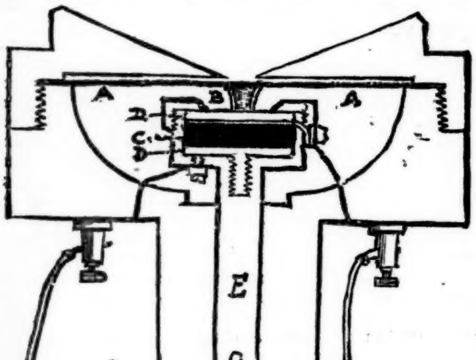

FIG. 2.—Section of Carbon Telephone—Latest form.

to entirely destroy the articulation of the magneto-telephone. Further, he has found the instrument practicable "*when included in a Morse circuit*, with a battery and eight or ten stations, provided with the ordinary Morse apparatus ; and that several way stations could exchange business telephonically upon a wire which was being worked, quadruplex, without disturbing the latter, and notwithstanding, also, the action of the powerful reversed currents of the quadruplex on the diaphragms of the receiver. It would thus seem as though the volume of sound produced by the voice with this apparatus more than compensated for the noise caused by such actions." Mr. Edison's assistant, Mr. Adams, now in England, states that conversation has been carried on during the night between New York and Chicago, places nearly 1,000 miles apart; and that under less favourable circumstances during the day the carbon telephone has been successfully used over a line of about half this length. Mr. Adams also informs the writer that at the Paris Exhibition he was able to transmit the music of a piano from Paris to Versailles, a distance of more than 20 miles; the piano standing 50 feet from the carbon telephone and yet not a note was lost at Versailles.

The present writer has had an opportunity of testing

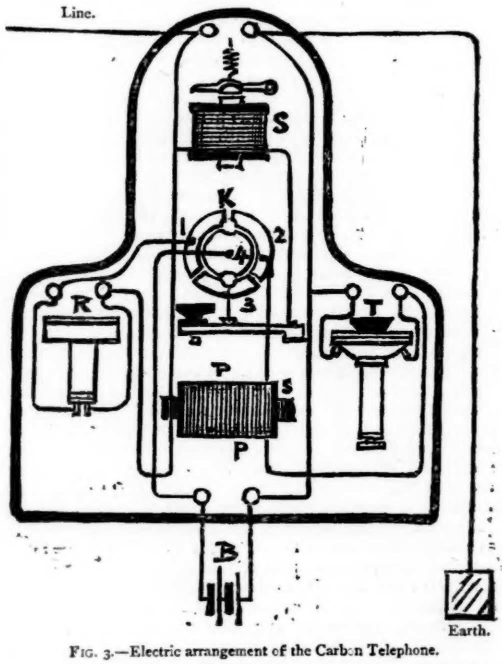

FIG. 3.—Electric arrangement of the Carbon Telephone.

this instrument in a recent lecture at the Midland Institute, Birmingham, and the surprising loudness of the tones received has been noticed in NATURE. Words spoken some thirty feet from the transmitter were clearly heard in the distant receiver, whilst loud speaking close to the transmitter enabled more than a hundred persons to hear simultaneously the words spoken when a paper cone was added to the receiver. And furthermore, single exclamations, such as Bravo! could be heard by the whole of a crowded audience of upwards of a thousand people. Even when the line wire was broken, the fractured ends being near to, but not touching each other, conversation could still be carried on through the circuit with the carbon telephone, though communication by the magneto-telephone and the ordinary telegraphic instruments was entirely interrupted. The writer has also just made further and more severe trials with this instrument on, he believes, the longest private wire in constant use in England, namely, that belonging to Messrs. Colman,

of Norwich and London, which firm have before this kindly allowed their wire to be freely used for experimental purposes. This wire stretches from Messrs. Colman's works at Norwich to their office in Cannon Street, a distance of a little over 115 miles. The wire runs on the same poles as the numerous other wires of the Great Eastern Railway, and is carried overhead from the terminus in London to Cannon Street. At 4 o'clock the experiments began, and the incessant crackling and bubbling sounds in the receivers revealed the fact that the adjoining telegraph wires were at their busiest, and that induction could hardly be worse. Nevertheless, the first exclamation uttered into the hastily adjusted carbon telephone at Norwich was heard perfectly in the

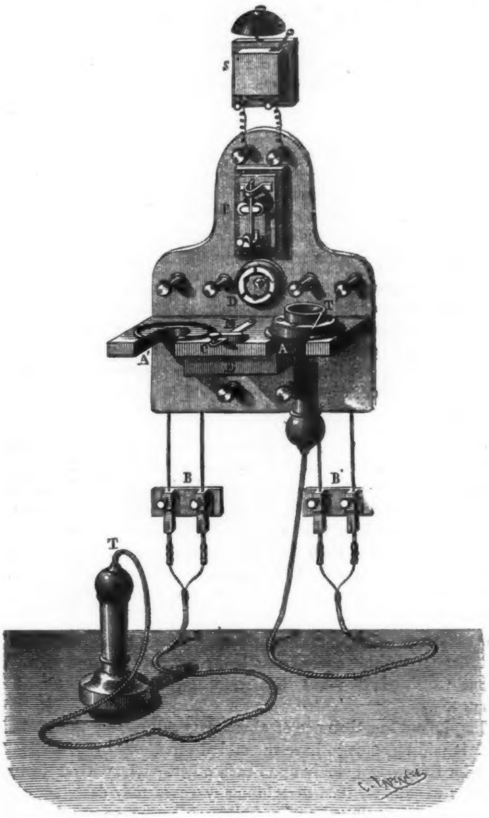

FIG. 4.—General view of the arrangements and accessories of the Carbon Telephone.

counting-house at Cannon Street. Conversation then ensued between the two places; some words were occasionally lost, but the American accent of Mr. Adams, Mr. Edison's professional assistant, who had charge at the Norwich end, was distinctly recognisable in London. Remarks passed on the weather showed that a storm of snow and sleet was going on at both ends, and the insulation therefore almost at its worst. Later on, towards 9 o'clock in the evening, the effects of induction grew less, but were still considerable. The voices from Norwich were now louder, the individuality of the speakers more marked, and conversation could be carried on without difficulty, the voices of certain speakers being remarkably distinct.

Twelve months previously the writer had an opportunity of trying Bell's telephone on the same circuit, when not a word could be transmitted during the day, owing to induction, but at night everything was clearly heard; hence the foregoing experiments established the important fact that in spite of powerful induction operating against it, the Edison telephone is a practicable instrument. It is true that before this telephone can be commercially used, especially during the day and on long lines, special electrical adjustments of the instruments must be made such as the pressure on the carbon and probably the resistance of the induction coil relatively to the line, but in this there is no inherent difficulty, and the adjustment once made no further change is likely to be necessary. Meanwhile we shall await with curiosity the new receiver, which, in a recent letter to the writer, Edison says will arrive in England soon; they differ from other telephones in having "no ear pieces or magnets about them," and according to Edison, "are about twenty times louder than any magnetic telephone, and can, if desired, reproduce the voice at the distant end louder than originally spoken, whilst the whole affair is even cheaper and simpler than the receivers now in use."

It is not impossible that before very long, by means of the Edison telephone, speeches in Parliament may be telephonically transmitted to the newspaper offices and to the country, whilst honourable members, if their articulation be distinct, are speaking from their ordinary places in the House.

W. F. BARRETT

NOTES

THE Corporation of Penzance are, we hear, making preparations to celebrate the centenary of Sir Humphry Davy's birth next month. The Paris Academy of Sciences, who awarded Davy a prize in 1807, when war was raging between France and England, will probably take some part in the celebration.

A COMMITTEE has been formed at Heilbronn with the object of erecting a monument to the memory of Dr. Robert Julius Mayer in his native place. Every one knows that Dr. Mayer's name is associated with the establishment of the mechanical equivalent of heat (see NATURE, vol. xvii. p. 450).

THE Observatory of Geneva has received the gift of an instrument of large dimensions by the generous munificence of its director. Prof. Emil Plantamour, who has occupied this position for about forty years, has constructed, at his own expense, in the existing building, a turret of 7 metres in diameter, surmounted by a cylindrical cupola, in which will be placed an equatorial telescope of 10 French inches aperture and 3·70m. focal distance. The object-glass has been ordered from Merz, of Munich, and the equatorial mounting is being manufactured at the workshop of the Geneva Society for the Construction of Physical Instruments. It is hoped that the new instrument will be in working order about the end of next spring.

DR. CREIGHTON, Demonstrator of Anatomy at Cambridge, has joined the editorial council of the *Journal of Anatomy and Physiology*, which henceforth adds to its title the words "normal and pathological."

WE would call the attention of our readers to a paper, likely to be of some interest, to be read at the meeting of the Physical Society on Saturday, by Messrs. Ayrton and Perry. The title of the paper is "The Music of Colour and of Visible Motion," and from what we can learn of Messrs. Ayrton and Perry's investigations, they claim to have hit on a new emotional art. By means of a new machine which they have devised they can produce combinations of harmonic motions with greater variety than can be obtained with any existing machine. Their idea, we believe, is that, judging from their experience partly of the feelings produced by large bodies in rapid motion and partly from the fact that in Japan posturing takes the place of the operatic

singing of the West they think that colour and motion may be made to produce emotions like good music, and therefore may very likely be employed as adjuncts in the entertainments of the future intended to work on the emotions.

AT a meeting of the Medical Society at University College, Gower Street, on December 3, at 8 P.M., an address on "The Use of Physiology to Medical Students," will be delivered by Dr. Michael Foster, F.R.S.

AT the monthly meeting of the Linnean Society of New South Wales, on September 30, the committee appointed to consider Baron Miklucho-Maclay's suggestion for the establishment of a zoological station at Sydney, presented their report affirming the desirability of forming such an institution. The Hon. W. Macleay having very liberally offered to afford facilities for a temporary zoological station in the vicinity of his residence, and promising the use of his museum, library, and microscopes to students of natural history, the Society adopted the report, and it was agreed to commence operations at once.

FROM a promising new American fortnightly journal, *Science News*, edited by Messrs. W. C. Wyckoff and E. Ingersoll, we learn that Prof. S. P. Langley, director of the Allegheny Observatory, has just started on a voyage to Europe, being commissioned by the United States Coast Survey to make observations to serve as a standard of comparison in determining the requisites for astronomical stations in American territory. The inquiry will have particular reference to the effects of different elevations and atmospheric conditions upon the fitness of various localities for the practical work of astronomy. Prof. Langley goes direct to Paris and thence to Italy; the trip will include an ascent of Mount Etna. In addition to the routine work of the Alleghany Observatory, Prof. Langley has been busily engaged in completing a direct experimental comparison between the heat of the sun and the highest heats attainable in the arts. The results indicate that the sun's *intrinsic* heat is almost beyond comparison greater than that of any blast furnace, and far larger than was reckoned by the French physicists. Prof. Langley has also nearly finished a memoir embodying great numbers of measurements and drawings at the extreme lower end of the solar spectrum, particularly the A group. These are parts of a research supported by the Rumford fund, requiring also a new study of the distribution of energy in the spectrum, as shown by the thermopile: Prof. Edison's tasimeter will probably be tested in the course of the work, using the Rutherfurd gratings to supply the spectra. A great improvement, Prof. Langley hopes, has recently been made by him in the accessories of the diffraction spectroscope, by means of which the use of collimators of extraordinary length will become practicable.

MR. C. E. ALLAN writes that he has constructed a rough pencil microphone, using cinders instead of carbon. This construction was not sensible to small sounds, but speech was transmitted very clearly. The pressure at the points of contact was increased by winding wire round the cinder pencil, and by this means the jarring sound of the cinders was almost totally removed, so that songs, the notes of an organ, and the ordinary tones of the voice were distinctly transmitted.

THE annual exhibition of the Haggerston Entomological Society takes place at 10, Brownlow Street, Dalston, to-day and to-morrow, at six P.M. This exhibition is always well worth a visit.

A CORRESPONDENT in the *Times* states that Mr. Edison, in reply to a telegram, avers that, according to his system, the altering of one light does not in the slightest degree affect others in the same circuit. He can adjust the brilliancy of each light at pleasure, it is stated, so that thus electrical lighting would be as steady and as much under control as gas itself. "Whatever method he uses, Mr. Edison appears confident of the success of his system." Mr. Sawyer, being interviewed by a New York reporter on the subject of his lamp, said he did not claim the incandescence of carbon in a sealed receiver as an idea of his own, but had utilised it in the development of other ideas. He claimed a form of conductor which would radiate the heat produced by the incandescence in such a way that the globe would be heated only at the point where the light was. He had also patented a process for charging the receiver with pure nitrogen gas and entirely displacing the atmospheric air. He had patented a means of so closing the receiver that no atmospheric air could find its way into the interior. The nitrogen, he claimed, would last for ever. Besides, there was a substance in the bag at the bottom of the lamp which would absorb oxygen and carbonic acid gas. These, he said, were his improvements. One lamp had been in use two or three hours daily for three months, until a sudden jarring of a door broke it. There had never been any flaking or change in the carbons used. The only change the carbon underwent was its purification. Bef ore being lit it was a dead black; after being incandescent for a time it took on the silver gray colour characteristic of pure carbon. The sub-division of the light, as produced by Mr. Sawyer's system, consists of branch wires leading from the two main wires of the engine. Each of these branches is calculated to supply a number of lamps. The extension of the main wires necessitates an increased heaviness of the wire for each mile. In lighting New York a radius of only half a mile from each supply station will be actually necessary.

THE Liverpool Corporation are taking steps to utilise the electric light as a public illuminating medium as soon as it is utilisable. They intend to apply to Parliament next session for powers to this purpose, have appointed a committee to watch over the subject, and have despatched their engineer to the Continent to examine into the use of the light in Paris and elsewhere.

A *Daily News* correspondent writing from Naples on the 13th inst. states that the stream of lava from Vesuvius was slowly extending from the cone towards the Atrio del Cavallo, the ravine or valley which separates Vesuvius from Somma. The stream extended almost the whole way into the Atrio del Cavallo, and divided into no less than three large streams. These were increasing in size and extent, and the slight shower of lava had also increased, but it was not sufficient to be observable from Naples.

AN earthquake took place at Sierra Leone on the morning of October 11, shaking every house in the colony, and causing great alarm to the inhabitants, but fortunately no damage of any moment was done. There were three successive shocks felt, travelling inland to a distance of about sixty miles, and the end of each is said by some to have resembled three very heavy peals of thunder following quickly upon each other. The natives in the interior were so terrified that in many cases they deserted their villages. An earthquake of a similar character occurred about fifteen years ago.

A M. BAILEY, of Paris, has invented an electric spark pen which possesses some points of interest. If a sheet of thin paper is attached to a plate of copper or zinc, it is stated that an engraving may be made with extraordinary facility by means of this pen. If one of the poles of a Ruhmkorff machine is attached to the plate and the other to the upper end of the pen, the current will run through, and in drawing the paper is perforated. When the drawing is finished, ink is laid on with an ordinary roller, and the greasy fluid penetrates through the holes. The plate is then plunged in water, which detaches the paper, and it is ready for immersion in the acid. The advantage claimed for this method is that the artist does all parts of his work and has no more trouble than if he were working with an ordinary pencil. He can even work in a dark room without any other light than the glare from the induction spark.

THE *Times* Geneva correspondent states that the remains of another lake village have just been brought to light at Lorcas by the shrinkage of the waters of the Lake of Bienne. This appears to be one of the most interesting discoveries of the sort we have had for some time, rich as have been the last few weeks in notable lacustrine finds. The station at Lorcas, assigned by experts to the age of stone, is situated at a short distance from the lake shore, not far from another and similar station which was explored in 1873. An exploration, conducted by Dr. Gross, of Neuveville, has resulted in the gathering of many novel and interesting objects, pierced stone hatchets similar to those found in Denmark, large flint lance-heads, jade hatchets with stag-horn and wooden hafts fastened with pitch; vessels in wood, among others a colander, and a vase in a good state of preservation. Near these were found several arms and instruments of pure copper, a circumstance which points to the probability that intermediate between the age of bronze and the age of stone was a period when prehistoric man had not discovered the art of alloying copper with tin. This was the age of copper. Still more remarkable is a find of human skulls which bear unmistakable marks of having been trepanned. Round pieces have been cut out, doubtless after death, as is supposed, for use as amulets. In some instances pieces were cut from the craniums of living infants in order, as M. Broca conjectures, to let out the spirit by whose malignant influence they were afflicted with fits, convulsions, and other maladies. These bits of infants' skulls were sometimes used in a way of which an example has been found at Lorcas; they were put inside the heads of the dead to protect them from the wiles and assaults of evil beings in the world of spirits.

A PORTRAIT of the Rev. M. J. Berkeley has been presented to the Linnean Society; it was painted by Mr. Peale at the instance of some of Mr. Berkeley's friends.

M. ROUX has sent to the Society of Encouragement of Paris the results of his experiments on nitro-glycerine, from which it appears that bottles of tinned iron falling from a great height and breaking do not cause a dangerous explosion.

WE have received an interesting syllabus of a course of ten lectures on literary and scientific subjects, to be delivered in the lecture theatre of the Bristol Museum, during the winter.

MR. BROTHERS, photographer, Manchester, asks us to state that the portrait of Sir George Airy, which we gave in a recent number, was from a photograph by him. The copy from which our portrait was taken did not indicate by whom it was photographed.

THE *Yama Sentinel* of California gives an account of a singular specimen of meteoric iron, which resembled steel, that had been found in the Mohave desert. It weighs about a pound, has some free gold on the surface, is not magnetic, and has successfully resisted the action of various acid baths. One of its surfaces shows a fracture of crystalline appearance, the colour of which is steel gray tinged with yellow. It has defied the best cold chisels, and has neither broken nor chipped under heavy blows. If its composition could be imitated it would be the hardest and toughest alloy known.

BEING at Osaka recently, a correspondent of the *Kobe Advertiser* was invited to inspect the cotton-mills and spinning-factory which was established at Sakai some years ago, but has attracted little notice. The account which he gives of his visit furnishes additional testimony of the progress which the Japanese are so rapidly making. The premises in question cover 7,000 tszuboes of ground, and the buildings thereon are substantially and well built, and the greater part of the machinery was imported from England. It is not necessary to enter upon the description of the internal arrangements of the establishment, but it is interesting to learn that "in this factory are employed about 150 hands, some 60 men and boys, a few elderly females, and about 80 girls. These latter resemble much the factory girls at home; the same merry countenances and laughing twinkling eyes, unabashed, but perfectly orderly, though perhaps a little negligent upon the appearance of visitors. . . . . We were highly gratified with our visit, showing as it did that there is a wide and very hopeful field for the development of industries in Japan."

WE have on our table the following books:—"Mathematical Drawing Instruments," by W. Ford Stanley, E. and F. N. Spon; "Crystallography," Henry Palin Gurney, S.P.C.K.; "Outlines of the Geology of Northumberland," G. A. Lebour, H. Sotheran and Co.; "Coal and Iron in all Countries of the World," John Pechar, Simpkin and Co.; "Abriss der praktischen Astronomie," Dr. A. Sawitsch, Wilhelm Manke; "A First Catechism of Botany," John Gibbs, Durrant; "The Present State of Electric Lighting," J. N. Schoolbred, Hardwick and Bogue; "The House-Surgeon," Alf. Smee, F.R.S., Accident Insurance Company; "The Geological Record for 1876," edited by William Whitaker, Taylor and Francis; "Etna," G. F. Rodwell, Kegan Paul and Co.; "Spiritual Science," Kuklos, John Harris; "Instructions for Testing Telegraph Lines," Vol. I., Louis Schwendler, Trübner; Health Primers—"Premature Death—Alcohol," "Exercise and Training," "The House," Hardwick and Bogue; "Gegenbaur's Elements of Comparative Anatomy," translated by F. Jeffrey Bell and E. Ray Lankester, Macmillan and Co.

MR. E. P. RAMSAY, Curator to the Australian Museum, Sydney, has prepared and issued "Hints for the Preservation of Specimens of Natural History for Museum Purposes." This short pamphlet contains useful directions for unskilled taxidermists, and notes on the preservation of entire animals of small size. It contains occasional remarks on Australian animals, and suggestions specially appropriate to the wants of naturalists in the bush; these are the only novelties. It will be seen that the title is rather too comprehensive for the contents of the paper; and now that we are beginning to look a little beyond the mere collection of dried skins, it is disappointing to find the internal organs of animals treated as so much matter to be got rid of.

THE last volume of *Medical Reports*, issued by order of the Inspector-General of Customs in China, contains a contribution of considerable interest to our knowledge of the geographical distribution of disease. The notes we refer to, which are by Mr. E. Rocher, of the Customs' Service, prove that the plague exists in China, and that it has in late years spread over a larger area than is generally known. They also show that the disease did not, as some believe, entirely disappear between 1844 and 1873, and it is thought by no means improbable that it may have passed from Yünnan to Mesopotamia or Persia. In Yünnan the disease is known as *Yang-toze*, and is believed to have been originally imported from Burmah. When that was it is impossible to determine, but since the commencement of the civil war it has spread over the whole province, decimating the population. There is one fact which inclines Mr. Rocher to think that the epidemic is owing to exhalations from the soil, viz., that those animals which live in the ground, in drains, or in holes, are the first to be attacked, and this is particularly noticeable with rats. As soon as these animals are ill they leave their holes in troops, and, after staggering about and falling over each other, drop down dead. The same phenomenon occurs in the case of other animals, such as buffaloes, oxen, sheep, deer, pigs, and dogs; all are attacked, but the dog less severely than the others. When these phenomena appear it is not long before the disease spreads to man, and, knowing this, the people do

what they can to guard themselves against it. They begin to purify their houses by lighting fires in every room, and in certain towns they abstain from pork. Mr. Rocher gives details as to the symptoms and course of the disease. With regard to the track of the epidemic Mr. Rocher observed a peculiar fact both in the north and south of the province. Instead of visiting every village in its direct line of progress it would pass some completely by, visiting places near them and on both sides, to return to those forgotten spots several months afterwards, when the epidemic would appear to have passed far away. Another fact not less curious is that after having appeared in almost every one of the villages scattered about the plains, it frequently ascends the mountains, where, among the aborigines who inhabit the high lands, it claims many victims. We may add that Mr. Rocher's notes are accompanied by a map, compiled from private and official memoranda, which shows the course followed by the plague in 1871, 72, and 73; it was not possible, however, to include in it the towns in the west of the province, which was at that time the theatre of the war between the Imperialists and the Mahometan rebels, as the information obtainable was quite untrustworthy, but it is certain that the epidemic was constantly present among the Imperialist troops.

THE additions to the Zoological Society's Gardens during the past week include a Green Monkey (*Cercopithecus callitrichus*) from West Africa, presented by Miss G. E. Marryat; a Bonnet Monkey (*Macacus radiatus*) from India, presented by Mr. F. Hinde; two Horsfield's Tortoises (*Testudo horsfieldi*) from Turkestan, presented by Dr. Alex. Strauch, C.M.Z.S.; a Wanderoo Monkey (*Macacus silenus*) from Malabar, two Egyptian Jerboas (*Dipus ægyptius*) from Egypt, a Sun Bittern (*Eurypyga helias*) from South America, deposited; a Woodcock (*Scolopax rusticola*), European, purchased.

CHARLES ADOLPHE WURTZ

IN connection with the Faraday Lecture which follows, it may interest our readers to have a few particulars as to the life and work of the lecturer, Prof. Wurtz.

Charles Adolphe Wurtz was born at Strassburg on November 16, 1817. He commenced his chemical career as assistant to Dumas, and first acquired an independent position as professor at the Agricultural Institute at Versailles. For the last thirty years he has been Professor of Chemistry at the École de Médecine, Paris; in addition to which he now holds the post of Professor of Chemistry at the Sorbonne.

Prof. Wurtz is a member of the Institute (Académie des Sciences), and a foreign Fellow of the Royal Society.

Some idea of the energy which he has displayed as an investigator is conveyed by the fact that a list of no less than seventy-three titles of papers is appended to his name in the Royal Society Catalogue, which only includes papers published previous to 1864. Much of his work is of the first importance in connection with chemical theory, and he undoubtedly takes rank as one of the chief pioneers of modern organic chemistry.

His first investigation, published in 1842, was on the constitution of the hypophosphites; this was followed by researches on phosphorous acid, sulpho-phosphoric acid, &c., which greatly added to our knowledge of the phosphorus compounds. It was in the course of his experiments on the hypophosphites that Wurtz discovered hydride of copper, Cu_2H_2, one of the most remarkable hydrides with which we are acquainted, and especially interesting as, with the exception of potassium, sodium, and perhaps palladium, none of the metals appear to be capable of combining with hydrogen. Hydride of copper is formed as a yellowish precipitate on adding a concentrated solution of copper sulphate to a solution of hypophosphorous acid, and warming the mixture to about 70° C.; in the dry state it slowly decomposes into its constituents at about 55° C.; concentrated hydrochloric acid at once dissolves it with evolution of hydrogen, although copper is not in the least affected by this acid, and what is most remarkable, both the hydrogen of the acid and of the hydride of copper are given off as shown by the equation—

$$Cu_2H_2 + 2HCl = Cu_2Cl_2 + 2H_2.$$

The study of certain cyanogen compounds—the cyanic and cyanuric ethers—next engaged his attention, and his researches on these bodies culminated in the remarkable discovery, in 1849, of the so-called compound ammonias formed by the displacement of one of the atoms of hydrogen in ammonia, NH_3, by organic radicles, such as methyl, CH_3, ethyl, C_2H_5, &c.

A third investigation to which we may here refer is that on the alcohol radicles published in 1855. Frankland had shown that the hydrocarbon radicles which it was assumed were contained in the alcohols could actually be isolated; that, from ordinary or ethyl acohol, for example, which may be regarded as a compound of the radicle ethyl, C_2H_5, with the radicle OH, we may obtain ethyl by acting with zinc on the iodide which it yields on treatment with hydriodic acid, thus withdrawing the iodine from it, just as the iodine is withdrawn from the hydrogen in hydriodic acid by the action of metals; and Kolbe had obtained similar results with acids, such as acetic acid, by submitting solutions of their salts to the action of a powerful electric current. These chemists, however, supposed that the radicles thus withdrawn from combination with other radicles remained in the free state, but Laurent and Gerhardt, and Hofmann argued on theoretical grounds that the bodies thus produced were not the radicles themselves but compounds of the radicles with themselves—that ethyl, for example, was not C_2H_5, but C_4H_{10} or $C_2H_5 + C_2H_5$. Conclusive evidence of the correctness of this latter view was afforded by Wurtz's discovery that if a mixture of the iodides of two different radicles were treated with metallic sodium, a hydrocarbon formed by the union of the two different radicles was obtained. This discovery has afforded one of the chief arguments in favour of the view now almost universally entertained by chemists, that free hydrogen is a compound of hydrogen with hydrogen.

The mere recapitulation of the titles of his remaining investigations would alone occupy a large amount of space. We can only refer to those on the glycols and on ethylene oxide; on the action of nascent hydrogen on aldehyde; on the action of chlorine on aldehyde, both in the anhydrous state and in presence of water; on the action of hydrochloric acid on aldehyde; on the synthesis of neurine; and on abnormal vapour densities, as being, among others, of especial interest.

ON THE CONSTITUTION OF MATTER IN THE GASEOUS STATE[1]

LADIES AND GENTLEMEN,—

I ESTEEM it a great honour to address you within these walls, about which there still hovers the ever fresh memory of him whose name we celebrate to-day, while we deplore his loss. I am fully sensible both of the great value of this honour and of the danger that attends it, and I have need to shelter myself under the authority of the great name of FARADAY. I have, therefore, chosen a subject connected with his earliest discoveries. The constitution of matter is a question of the highest importance with regard both to physics and to chemistry.

The word "gas" was introduced into science by Van Helmont, who, at the beginning of the seventeenth century, first pointed out, with some degree of precision, the differences existing be-

[1] The Faraday lecture, delivered before the Fellows of the Chemical Society, in the Theatre of the Royal Institution, on Tuesday, November 12, 1878, by Ad. Wurtz, Membre de l'Institut; Doyen Honoraire de la Faculté de Médecine de Paris.

tween certain aëriform fluids. He it was who first spoke of *Gas silvestre*, formed by the combustion of charcoal, and given off during the fermentation of beer. To him, also, we owe the distinction—which kept its ground for two centuries—between gases and vapours. He regarded gases as aëriform fluids, incapable of reduction to the liquid state by cooling, whereas vapours require the aid of heat to maintain them in the gaseous state. An important difference of constitution seemed, therefore, to exist between these two kinds of aëriform fluid. This difference, however, is not fundamental, and the distinction between gases and vapours has disappeared, in a theoretical point of view, being, in fact, reduced to a simple question of temperature and pressure.

On March 13, 1823, Faraday, then a young man engaged as chemical assistant at the Royal Institution, read before the Royal Society a note entitled "On Fluid Chlorine." He had succeeded in condensing this gas to a liquid by a process which has become classical. This process consists in heating in a closed vessel placed in a water-bath crystals of chlorine hydrate. This compound, very rich in chlorine, is resolved at a gentle heat into chlorine and liquid water, the quantity of which is not sufficient to dissolve the whole of the chlorine. The latter is therefore disengaged in great part in the state of gas, which accumulates in the small space remaining to it, and is liquefied by the pressure which it exerts upon itself.

On the same day Sir Humphry Davy read a note "On the Liquefaction of Hydrochloric Acid Gas," which he effected by decomposing sal-ammoniac with sulphuric acid in a closed vessel. These researches were completed by Faraday, who, on April 10 of the same year, described the liquefaction of a large number of gases, directing his efforts, by Davy's advice, chiefly to those which are dense, or very soluble in water, such as sulphurous acid, ammonia, sulphuretted hydrogen, carbonic acid, and protoxide of nitrogen.

To enumerate the special processes adopted in each particular case would occupy too much time. We shall therefore merely observe that the chief, if not the only, means of condensation adopted in these experiments was compression, that is to say, the reduction of the gas to a small volume, and that this compression was exerted by the gas upon itself, as it accumulated in the very strong sealed glass tubes in which it was disengaged. Sir Humphry Davy, in the note above cited, had remarked that pressure appeared to be a more efficacious method of condensation than cooling, inasmuch as a double pressure reduces the volume of the gas to one-half, whereas a depression of temperature of 1° F. reduces the volume by only $\frac{1}{480}$, the lowering of temperature, moreover, soon attaining an impassable limit. It must, however, be especially observed that, even in his first experiments, Faraday made use of differences of temperature, if not to liquefy the gases, at all events to distil and isolate the liquids. Thus it was in the case of chlorine, for example, and in that of ammonia, which he liquefied by heating ammoniacal silver chloride in a bent tube sealed at both ends, the liquid ammonia then distilling over and collecting in the empty branch of the tube, which was cooled to a low temperature.

Similar phenomena will be exhibited in the experiment which I am about to show you, consisting in the liquefaction of c y a n o g e n g a s by heating cyanide of mercury in a small glass tube terminated by a long capillary tube bent in the form of the letter U. The figure of this curved portion will be projected on a screen by the electric light, and in a few seconds you will see the liquid cyanogen collect in the bend.

Before leaving this part of my subject, I would recall to your attention two of Faraday's discoveries resulting from the application of the principles just explained. Having compressed coal-gas to twenty-five atmospheres, Faraday in 1825 discovered two important bodies, namely, b u t y l e n e, a compound of great importance in a theoretical point of view—and b e n z e n e—so named by Mitscherlich several years afterwards—which in our own time has become the object of numerous and important applications, and the pivot of an entire department of chemistry.

Another instance is afforded by s u l p h u r o u s a c i d g a s (SO_2), which was liquefied by Bussy in 1824, at the ordinary atmospheric pressure, by the effect of a cold of 12° to 15° below zero.

Whether we condense gases by pressure or reduce them to the liquid state by diminution of temperature, the result of either method is to bring their particles closer together. It would seem then, in accordance with Davy's view, that pressure ought to be more efficacious, as a means of condensation, than cooling. Nevertheless it is not so. The mere approximation of the particles of certain gases does not suffice to effect their liquefaction, and moreover, the distances between the particles cannot be diminished indefinitely by pressure alone. M. Natterer, of Vienna, has compressed oxygen, hydrogen, and nitrogen to 3,000 atmospheres without effecting their liquefaction. These gases, hitherto called permanent, cannot be liquefied by pressure alone, and their liquefaction, which has quite recently been effected, is the joint effect of strong pressure and a great degree of cold. This is the important point, and I request your permission to offer in this place a few explanations which will serve to place it in its true light.

The impossibility of liquefying certain gases by pressure alone is in accordance with the ideas which are current at the present day respecting the nature of aëriform fluids, and likewise with a discovery made in England within the last few years, on the continuity of the gaseous and liquid states. I will explain myself briefly on these two points.

Daniel Bernouilli first enunciated the idea that gases are formed of material particles, free in space, and animated by very rapid rectilinear movements, and that the tension of elastic fluids results from the shock of their particles against the sides of the containing vessels. Such is the origin of the k i n e t i c t h e o r y o f g a s e s, which has been revived since 1824 by Herapath, Joule, and Krönig, and developed chiefly by Clausius and Clerk Maxwell.

The law of Boyle and of Mariotte follows as a natural consequence of this idea. Suppose a gas occupying a certain volume, and composed of a definite number of material particles—or molecules so-called—to be contained in a closed vessel, such as the cylinder of an air-pump; the pressure on the piston will be determined by the number of shocks of the molecules diffused through the neighbouring stratum of gas. If, then, the volume of the gas be reduced, the number of particles in this layer will be increased, as well as the sum of the shocks, and the pressure will be increased in proportion thereto.

The velocities with which these molecules move are enormous. Clausius supposes that the molecules of air move with a mean velocity of 485 metres per second, and those of hydrogen with a mean velocity of 1844 metres per second. I say mean velocity, for all the particles of a gas do not move at the same rate. But can the particles freely traverse these wide spaces? By no means; their number is so immense, that at every instant they enter into collision with one another, and rebound in such a manner that their motion is altered both in velocity and in direction. It follows, therefore, that the molecules of a gaseous mass are continually moving in all directions with variable velocities, their motion in the intervals between the collisions being sensibly rectilinear. The distribution of the velocities has been made the subject of important researches by Clerk Maxwell.

These movements of gaseous molecules determine a very important physical condition, namely, t e m p e r a t u r e. In fact, the energy of the rectilinear movements, that is to say, the mass of the gaseous molecules multiplied by the square of their velocity, gives the measure of the temperature, which consequently increases proportionally to the energy of the rectilinear movement, or, for the same gas—since the masses remain constant—it increases as the square of the velocity. If the velocity were reduced to nothing, the calorific motion would be annihilated, that is to say, the gas would be entirely deprived of heat. This state corresponds with the absolute zero.

The gaseous molecules moving in all directions and coming into collision with one another in space, are very nearly emancipated from cohesion. Nevertheless this attractive force makes itself felt for the infinitely short time during which the molecules actually touch one another, or are on the point of doing so. This influence of cohesion is one of the causes of deviation from the law of Boyle or of Mariotte.

In liquids the influence of cohesion is manifest, preventing the molecules from separating, though it allows them to glide one over the other. This molecular cohesion, or attraction, is in continual strife with the force of expansion, or kinetic energy, which, if unopposed, would launch the molecules into space.

To understand the antagonism between these two forces, consider for a moment a saturated vapour in contact with the liquid from which it has been formed. When it is reduced to a smaller volume, a certain number of its molecules are brought within the sphere of action of cohesion; they are consequently aggregated together and precipitated in the liquid state, while the rest, being

now diffused through a wider space, continue to move with the same velocity and to exert the same pressure as before. In this case the force of cohesion of the liquid particles exactly balances the expansive force or kinetic energy, and serves to a certain extent as a measure of its amount.

Now let the vapour be heated, after it has been withdrawn from the action of the liquid; its expansive force will then increase; it will dilate, and may then be compressed, until, by the approximation of its particles, it is again brought within the sphere of action of the cohesive force, that is to say, to the point of saturation corresponding with the temperature to which it has been raised. With the increase of temperature, the expansive force or kinetic energy of the vapour likewise increases, whereas the cohesion of the liquid becomes less: hence the necessity of further diminishing the distances between the particles by increase of pressure. But this double effect of increased kinetic energy of the gaseous molecules, and diminished cohesion of the liquid molecules, going on progressively as the temperature rises, a point will at length be attained at which the energy of the molecular movement will finally gain the victory over the force of cohesion, *whatever be the pressure to which the vapour is subjected*. The minimum temperature at which this effect is produced, and at which, therefore, a vapour can no longer co-exist with its liquid under any pressure whatever, has been called by my friend, Dr. Andrews, the critical point, and by M. Mendelejeff, the absolute boiling point. Above this temperature, whatever may be the pressure, the gas, whether dilated or compressed, will maintain the same physical state, [characterised by freedom of molecular or calorific movement.

I can show you by an experiment this peculiar phenomenon of the sudden passage of a liquid mass to the state of gas, by heating liquid carbonic acid in a closed vessel, just as Cagniard de Latour formerly heated ether. Here is a tube, half filled with liquid carbonic acid, which we are about to immerse in water at 35°; you observe that the liquid first rises quickly in the tube, its coefficient of expansion being greater than that of gases; at the same time the meniscus flattens more and more, indicating a diminution of cohesion in the liquid (Andrews), and finally disappears altogether; in fact the liquid itself has disappeared, having been entirely and suddenly transformed into gas. What now must we do to cause it to reappear? We must lower the temperature, so as to diminish the kinetic energy of the gas, and increase the cohesion of the liquid. A moment will then arrive when the cohesive force will again be able to resume the contest, and the liquid will be reconstituted.

We are now in a position to understand why certain gases, hitherto called *permanent*, cannot be liquefied except by the combined action of very strong pressure and a very great degree of cold. The critical points of these gases are situated at very low temperatures. They have quite recently been liquefied, this great discovery having been made by MM. Cailletet and Raoul Pictet.

The principle of Cailletet's apparatus is the following:—The gas to be liquefied is introduced into a cylindrical glass vessel and transferred by means of mercury to a very strong glass tube sealed into the reservoir. This latter is firmly fixed in a cylindrical cavity hollowed out of a block of iron, and serving as a kind of closed mercurial trough. The cylindrical cavity communicates with a hydraulic press which injects water on to the surface of the mercury, driving it into the gas reservoir, which is ultimately quite filled with that liquid, the gas being thereby driven into the tube, where it is liquefied.

In this manner we shall be able by a few strokes of the piston of the hydraulic press to liquefy carbonic acid. Other gases less easily condensable may be liquefied in a similar manner, if the tube be cooled to − 20° or − 30°. But these temperatures do not suffice for the liquefaction of the so-called permanent gases. To cool these gases to lower temperatures, M. Cailletet avails himself of sudden expansion (*détente*). The gas, compressed to several hundreds of atmospheres, when allowed to expand suddenly and drive the air before it, consumes a certain quantity of heat, and is thereby reduced to a kind of mist, which will appear on the screen, and pass away like a cloud, if we suddenly expand the strongly compressed carbonic acid gas, which we have here, in default of oxygen or hydrogen.

M. Raoul Pictet has succeeded in condensing oxygen and hydrogen in the form of liquids, properly so called, and even in obtaining the latter of these gases in the solid state. To produce this effect, he employs condensing apparatus of incomparable power, combining the action of a cold of 120° to 140° below zero with that of enormous pressures amounting to 550 and even 650 atmospheres. The pressure is produced by the accumulation of the gases in a closed space consisting of a long copper tube of very thick metal. The oxygen was produced by heating potassium chlorate in a howitzer shell, having a copper tube soldered into its orifice. The hydrogen was prepared in a similar apparatus, by decomposition of a dry mixture of potassium formate and potassium hydrate.

To produce very low temperatures of 120° or even 140° below zero, M. Pictet resorts to a very ingenious artifice. Over the reservoir-tube which surrounds the copper tube, and in which these low temperatures are intended to be produced, he superposes another system of concentric tubes, intended to produce a first fall of temperature amounting to − 65°, by the volatilisation of liquid sulphurous acid. By means of this first depression of temperature it has been found possible to liquefy carbonic acid gas in the inner tube of the system just mentioned, by a pressure of only a few atmospheres. The carbonic acid thus liquefied being introduced into the lower reservoir-tube of the apparatus, produces by its volatilisation, a second fall of temperature round the copper tube containing the compressed oxygen which is to be liquefied. M. Pictet has in fact established a double circulation, one of sulphurous acid, the other of carbonic acid. I will describe the former. Sulphurous acid gas is liquefied by a pressure of three atmospheres and collects in a strong vessel, from which it passes through a tube into the upper reservoir. The pressure is exerted by means of a force-pump. A suction-pump connected with the force-pump, and acting in concert with it withdraws the liquid sulphurous acid from the reservoir-tube, and transfers it to the force-pump, which brings it back to the vessel, and thence to the upper reservoir-tube.

The circulation of the carbonic acid is established in the same manner, by means of two pumps, one of which condenses the gas by forcing it into tubes cooled to − 61°, while the other, which is a suction-pump, sends it back to the force-pump. The volatilisation of the carbonic acid produces round the copper tube the low temperatures above-mentioned. The copper tube is in fact surrounded by solid carbonic acid.

In this manner M. Pictet has liquefied oxygen, and has approximately calculated its density. He has also liquefied and even solidified hydrogen, which he has seen to issue from the tube in the form of a steel-blue liquid jet, which partly solidified. The solid hydrogen, in falling on the floor, produced the shrill noise of a metallic hail, thus confirming the bold and ingenious idea of Faraday, who first suggested that hydrogen is a metal.

The experiments of MM. Raoul Pictet and Cailletet have then removed from science the distinction between permanent and condensable gases. Permanent gases exist no longer. All aeriform fluids may be liquefied with a facility greater in proportion as their critical points are situated at higher temperatures. From a physical point of view, therefore, gases and vapours have the same constitution, being formed of molecules which move freely in space. In what, then, do they differ? They differ by the nature and constitution of these molecules; and here we enter on the domain of CHEMISTRY.

It is supposed, in chemistry, that the molecules of each species of gas or vapour are formed of a definite number of atoms. The simplest molecules, like those of mercury-vapour, are formed of single atoms. Others include several atoms of the same or of different kinds: and these latter molecules may be very complex, that is to say, formed of a large number of atoms held together by affinity, and vibrating in concert in a system to which they are attached, viz., the molecule. In this system, which has a definite form, extent, and centre of gravity, the molecules execute their own proper movements, and are at the same time carried forward with the entire system in the molecular paths.

I cannot here dilate on the nature and chemical properties of the several gases and vapours. I wish merely to throw light on a single point, which is of great importance, inasmuch as it constitutes one of the foundations of chemical science.

The proposition which I am about to enunciate is generally adopted by chemists, resting as it does on an imposing array of facts: *Equal volumes of gases or vapours, under the same conditions of pressure and temperature, contain equal numbers of molecules.*

The Italian chemist, Amadeo Avogadro, in discussing the discoveries of Gay-Lussac respecting the simple relations which exist between the volumes in which gases combine, was the first to recognise that there likewise exists a simple relation between the volumes of gases and the number of molecules which they contain. The simplest hypothesis, said he, that can be made regarding this matter consists in supposing that all gases contain in equal volumes equal numbers of "integrant molecules." By this term he denoted what we now call simply *molecules*, and he distinguished these integrant molecules from the "elementary molecules" which we call *atoms*. According to him the integrant molecules of gases are all equally distant one from the other, and these distances are so great in proportion to the dimensions of the molecules, that the mutual attraction between the latter is reduced to nothing.

These integrant molecules are composed of a greater or smaller number of elementary molecules, not only in compound, but likewise in simple bodies; the integrant molecules of chlorine, for example, are composed of four elementary molecules, and the same is the case with the integrant molecules of hydrogen. What happens, then, when chlorine and hydrogen combine together? The integrant molecules of these two bodies are then resolved into elementary molecules, which combine, two by two, to form hydrochloric acid.

Ideas analogous to those of the Italian chemist were enunciated in 1814 by Ampère, and thus there has been introduced into chemical science the notion that there exist two kinds of small particles, namely, molecules and atoms, the former being diffused in equal numbers through equal volumes of gases.

But this notion, so clearly enunciated more than sixty years ago, was afterwards destined to be obscured. Berzelius, taking up Ampère's proposition, altered it by substituting atoms for molecules, and saying that "equal volumes of gases contain equal numbers of atoms." This proposition, which has given rise to long discussions, must now be rejected, for it is inexact. It is to Gerhardt, and more recently to Cannizzaro, that is due the honour of having restored the thesis of Avogadro and Ampère, and pointed out its importance in connection with chemical theory. This I must explain in conclusion.

In the first place Gerhardt simplified the rule of Avogadro. The latter supposed that a molecule of chlorine or of hydrogen contains four atoms, whereas Gerhardt regards it as consisting of two. Avogadro's proposition thus modified, assumes a very simple form, and may be enunciated in the following terms. Suppose that a volume, or the unit of volume, of hydrogen contains one atom; then the molecules of all gases and vapours will occupy two volumes. Thus, a molecule of hydrogen formed of two atoms will occupy two volumes, and a molecule of chlorine formed of two atoms will likewise occupy two volumes. What now will happen when chlorine combines with hydrogen? The molecules will be cut in two, and each of the two chlorine-atoms uniting itself to an atom of hydrogen, two molecules of hydrochloric acid will be formed, each occupying two volumes. Thus if an atom of hydrogen occupies one volume, a molecule of hydrochloric acid will occupy two volumes. The same is the case with the molecules of all other gases and vapours.

A molecule of water formed of	2 at. H and 1 at. O	occupies	2 volumes.
" ammonia "	3 at. H and 1 at. N	"	"
" marsh gas "	4 at. H and 1 at. C	"	"

This list might be prolonged by taking as examples a large number of gaseous or volatile bodies belonging both to mineral and to organic chemistry, and including chlorinated, brominated, and oxygenated compounds of the metalloids, and of a large number of metals. The countless volatile compounds of organic chemistry, hydrocarbons, alcohols, chlorides, bromides, organo-metallic compounds, compound ammonias, aldehydes, ketones—all this legion of various compounds—conform to the law of Avogadro and Ampère, their molecules occupying two volumes if an atom of hydrogen occupies one volume. Hence it follows that the relative weights of two volumes represent the relative weights of the molecules, or the molecular weights. To find these latter, therefore, it is sufficient to double the numbers which express the weights of a single volume, or of the unit of volume, that is to say the densities. The densities of gases may be referred to that of hydrogen as unity, and the atomic weights to that of hydrogen. The unit being then the same, it follows that the numbers which express the double densities referred to hydrogen will also represent the molecular weights.

Chemists represent the constitution of molecules by formulæ, each of which shows the number of atoms condensed within the molecule. Now the molecular weights being known, it is very easy to deduce the formulæ from them, as these formulæ must represent the number of atoms comprised in two volumes. Such is the relation which exists between the Law of Volumes and Chemical Notation. The rule of Avogadro and Ampère has, in fact, become one of the bases of this notation. There are, however, certain exceptions to its generality, but they are probably more apparent than real. Sal-ammoniac, ammonium sulphhydrate, phosphorus pentachloride, iodine trichloride, sulphuric acid, calomel, amylene hydrobromide, and chloral hydrate, have vapour-densities such that their molecules appear to occupy four volumes. Such, however, is not the case; and it may be shown that the bodies in question do not volatilise without decomposition, but that, when they are heated, their molecules split up into two, each of which occupies two volumes. Being unable to analyse all the cases above-mentioned, I will confine myself to the last, viz., chloral hydrate, which has given rise to a long discussion.

The question to be decided is, whether this compound is or is not decomposed by conversion into vapour? If it really suffers decomposition, it should be resolved into anhydrous chloral and water. That this decomposition really takes place may be shown by a method based on the theory of dissociation developed by M. H. Sainte-Claire Deville.

Here is the case in a few words. We have here in a tube a certain volume of the vapour of chloral hydrate under a certain pressure; it is required to show that this vapour contains vapour of water. For this purpose we are about to introduce into it a body capable of emitting vapour of water, crystallised potassium oxalate, for example. If the atmosphere is dry, this salt will give off vapour of water just as it would in dry air or in vapour of chloroform at the same temperature, and it will continue to emit this vapour until the atmosphere shall have taken up a degree of humidity corresponding with that which is designated by M. H. Sainte-Claire Deville the *dissociation tension* of the hydrated salt in question. If, on the other hand, the chloral atmosphere is moist, and exhibits exactly the degree of humidity just defined, the crystallised oxalate will not emit any water. In this first tube, then, we have the vapour of chloral hydrate; the second contains vapour of chloroform. This latter is dry, and I am about to prove to you that the former is moist. In fact, the crystallised potassium oxalate which we are introducing into the chloroform tube will rapidly depress the level of the mercury by emitting vapour of water, whereas in the atmosphere of chloral hydrate it will not emit vapour of water, and consequently will not depress the level of the mercury. This shows that chloral hydrate undergoes decomposition when converted into vapour, and this supposed exception to the rule of Avogadro and Ampère vanishes, like all the rest, when submitted to the test of experiment. This rule appears, then, like a grand law of nature, as simple in its enunciation as it is important in its consequences.

Such are the considerations which I wished to lay before you on the physical and chemical constitution of gases. Does not this exposition seem to show that, of all the states which matter can assume, the gaseous state is the most accessible to our researches, and the best known—not, indeed, that we can affirm the certainty of the theoretical considerations which I have brought before you, for they are but probable. In the physical sciences nothing is certain but well-observed facts and their immediate consequences; and, whenever we attempt to make these facts the basis of any general theory, hypothetical data are apt to mix themselves up with our deductions. In the present case the hypothesis consists in assuming that gases, and matter in general, are formed of molecules, and these latter of atoms. No one has ever seen these molecules and atoms, and it is certain that nobody ever will see them. Does it follow then that we ought to reject or disdain this hypothesis? By no means. Our theories may be verified in their consequences, and may thereby acquire a certain degree of probability. The theory under consideration has been subjected to this ordeal, and nothing has hitherto been found to contradict it. It is probable, indeed, that gases are composed of small particles moving freely in space, with immense velocities, and capable of communicating their motion by collision or by friction. It is probable that these molecules are diffused in space in numbers so enormous that the most rarefied spaces still contain legions of them; and it is this circumstance which explains the possibility of the movements of the radiometer.

Be this as it may, the idea of Daniel Bernoulli has been developed into a beautiful theory—the kinetic theory of gases—a theory which has shed a sudden clearness, an unexpected light, on matters which seemed to be veiled in the deepest obscurity. The molecules, as already stated, are invisible. Nevertheless, attempts have been made to penetrate this invisible world by the force of scientific reasoning, and by an effort which does honour to the human mind, even if it be destined to remain barren. The illustrious authors of the kinetic theory of gases have sought to determine, not only the velocities of the gaseous molecules, and the prodigious number of their collisions during a unit of time, but likewise their distances, their absolute dimensions, and their number in a given volume. And here we arrive at results which bewilder the imagination, but which, in this lecture, I must not attempt to unfold.

Permit me only to add that these great labours mark a resting place in our course, and are, perhaps, an approach towards the solution of the eternal problem of the constitution of matter—a problem which dates from the earliest ages of civilisation, and though discussed by all the great thinkers of ancient, as well as of modern, times, still remains unsolved. May we not hope that in our own time this problem has been more clearly stated and more earnestly attacked, and that the labours of the nineteenth century have advanced the human mind in these arduous paths, more than those of a Lucretius, or even of a Descartes and a Newton. From this point of view, the discoveries of modern chemistry, so well expressed and summarised by the immortal conception of Dalton, will mark an epoch in the progress of the human mind; and to one of the most important among these discoveries—that of the liquefaction of the gases—grateful posterity will for ever join the glorious name of FARADAY.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

BIOLOGISTS will be pleased with the frank recognition of Dr. Foster's services contained in the statute proposed by the Council of the Senate at Cambridge for the new Professorship of Physiology to be founded by Trinity College. It is to be permanently recorded that Dr. Foster's lectures have always been open to the entire University, and that he "has successfully promoted" the study of physiology. Thus the continued self-denying effort and enthusiasm which have in eight years developed a school of over seventy students, and which have called forth the original talents of a score of ardent young investigators, will find still fuller scope. We understand that Dr. Foster resolutely declined to sanction any arrangement by Trinity College to secure for him the first tenure of the professorship, preferring to leave the University perfectly unfettered in its choice. But the Council of the Senate, which is a thoroughly representative body, chooses to signify the inseparable connection of the work with Dr. Foster's name by the very wording of the statute. The professor is to be elected by a board consisting of the Vice-Chancellor, of four members nominated by the Council, and four nominated by the Board of Natural Science Studies. One of each four must be neither resident in the University nor officially connected with it.

THE Cambridge mechanical workshops, organised by Prof. Stuart, bid fair to become of importance to research in the country generally, as well as in Cambridge. Prof. Stuart, on his own responsibility, has completely fitted up the workshops with all machinery necessary for the construction of philosophical apparatus. He has engaged a number of the most competent workmen as teachers, and to construct apparatus required by professors and investigators who are often deterred from researches because of lack of appliances or time to make what they want. Classes are formed for the regular instruction of university men in the use of tools and the construction of machines, and these are attended at present by a dozen students, several of whom intend to become engineers.

MR. A. C. HADDON, of Christ's College, has been nominated by the Board of Natural Sciences Studies, Cambridge, to study at the Zoological Station at Naples during the ensuing season.

DR. GREENFIELD, of St. Thomas's Hospital, has been appointed by the Senate of the University of London to succeed Dr. Burdon Sanderson as Professor of Comparative Pathology at the Brown Institution.

THE subscriptions already received or promised for the extension of the buildings of University College, London, amount to upwards of 14,000*l.*

BY the will of the late Mr. Charles Randolph, engineer, 60,000*l.* has been left to the building fund of Glasgow University.

THE third annual report of the Johns Hopkins University, Baltimore, issued by President Gilman, is of the highest interest, and shows that the attempt to establish a purely philosophical university has been eminently successful. Our readers are no doubt familiar with the principles on which this institution has been based. It was not sought to add one more to the many colleges already existing in the United States, but to found a genuine university in which those who had the inclination and the capabilities would have every facility for carrying their elementary or collegiate studies into the region of research in the various departments of human knowledge. The method of work has been carefully planned; the best men obtainable have been got to superintend the work of the students, who are admitted only on showing that they are really able and willing to pursue the courses which have been arranged. It is a many-sided and active centre of the highest learning, and cannot but have an invigorating result on science in all its departments in the United States. We would recommend those of our readers interested in the higher education to procure a copy of this report, which deserves a more detailed notice than we have space for.

THE Budget for Public Instruction will be deposited this week in the Bureau of the French Chamber of Deputies. A large increase is asked for in favour of public instruction. The credit granted will exceed two millions sterling. In 1823 it was only two thousand pounds, consequently in a little more than half a century it has been multiplied a thousand-fold. M. Bardoux will propose the creation in each department of a high school for popular education according to the models which have proved so successful in Paris. The benefit of the organisation realised in the capital will be extended to the whole of France if the scheme of the active minister is adopted, as will most probably be the case.

AT Stockholm the "Free" University was opened on October 14 last. The funds collected for its foundation now reach the sum of 820,000 Swedish crowns. It is intended to establish a similar university at Gothenburg.

SCIENTIFIC SERIALS

The American Journal of Science and Arts, October.—Besides two valuable papers by Professors Mayer and Draper, reproduced in our columns, we have here an account of the curious artificial mounds of North-Eastern Iowa, by Mr. McGee. They consist of tumuli, smaller conical mounds, embankments, and animal mounds, and from numerous measurements the builders seem to have used a unit which either was, or grew out of, the pace or yard. A slow southerly migration of the mound-builders is supposed to explain the evident increase in geometrical knowledge attested by various works found in passing across the United States from north to south.—Prof. Young furnishes details of observations of the Princeton Eclipse Expedition.—The flour-mill explosion at Minneapolis in May was probably due to the running dry of a set of stones which ground middlings, one of six sets discharging into a spout which communicated with a dust-house. Mr. Peckham studies the case, pointing out that there is greater danger with middlings, because it is dryer, and is ground at a higher temperature, and finer. The dry stones may heat the last part of the grist remaining, sufficiently to make it like tinder, so that it readily ignites on receiving a spark from the stones. The practical problem is how to prevent or detect dry stones, especially those for middlings.—Mr. Becker indicates the *rationale* of correction for vacuum in chemical analysis.—Prof. Smith writes on the composition of the new meteoric mineral, Daubreelite, and its frequent, if not universal, occurrence in meteoric irons.—Prof. Watson gives a more careful determination (than previously) of the intra-Mercurial planets.

Annalen der Physik und Chemie.—No. 9, 1878.—The excitation of electricity on contact of solid and gaseous bodies, forms the subject of an opening paper by Herr Beetz, who thinks the case is either one of differences of tension produced by different conducting liquids, or of change of metals by gases which have ceased to be in the gaseous state, either through occlusion in the metals, or condensation on their surface.—From experiments on production of

galvanic currents by streaming of liquids through tubes, Herr Dorn infers that the motion of the liquid in itself produces no considerable part of the electromotive force observed; the influence of the tube wall, on the other hand, is undoubted.—Herr Wiedemann shows that an examination of magnetic behaviour of iron oxide salts is well adapted for determining with accuracy, even quantitatively, their dissociation in solutions at different temperatures, the conditions of their fixation by acids, and their exchange with other salts.—In the first portion of an inquiry into the divergences of some gases from Boyle's law at 0° and 100°, Herr Winkelmann gives an interpolation formula, expressing this divergence in the case of ethylene.—A new proposition in the theory of diffraction proved by Herr Frölich, is, that with small angles of diffraction, the kinetic energy of the incident light for an aperture of any shape is equal to the kinetic energy of the diffracted light. —Some experiments on the nature of the phases and change composition in telephonic transmission are described by Herr Hermann.—There are also notes on the relation between refraction equivalent and wave length, and on excitation of electricity by pressure and friction.

Journal of the Franklin Institute, October.—This contains a short account by Prof. Henry Draper, of his eclipse observations at Rawlins, Wyoming Territory, together with a photograph of the corona, showing the unequal distribution of its matter in the plane of the ecliptic and ray-like forms towards the poles of the sun.—Mr. Bell furnishes an account of the now historic "Camel" locomotive engine of Ross Winans, built in June, 1848. It first practically demonstrated the superiority of the eight-wheel connected engine for heavy traffic; it had also an inclined firebox, and other features of novelty.—The new system of electric lighting by Profs. Thomson and Houston, is described, consisting in causing one or both the carbon electrodes to vibrate to and from each other, so that the effect of the light produced is continuous. This allows of a feebler current being used.—Mr. Isherwood analyses some Scotch experiments on economic vaporisation of water and expansion of steam.

SOCIETIES AND ACADEMIES

LONDON

Linnean Society, November 7.—Prof. Allman, F.R.S., president, in the chair.—Sir Joseph D. Hooker, C.B., presented to the Society, in the name of a committee of gentlemen, a portrait of the Rev. M. J. Berkeley. A great matted mass in sheet of a Chara (*Nitella* sp. ?) was exhibited by Dr. Thos. Boycott. It had been got from a dried-up pond in St. Leonard's Forest, Sussex, June, 1877; within its meshes many interesting microscopic forms were obtained.—Mr. Thos. Christy next called attention to living specimens of West African indian-rubber trees, the *Urostigma Vogelii*, and another undetermined species recently arrived. He likewise showed the fruit, flower, and leaf in spirit, with a dried ball of the gum of the commercially valuable *Landolphia florida*.—Dr. Maxwell Masters read an extract from a letter of Dr. Beccari describing a gigantic Aroid found by him in Sumatra, side by side with the *Rafflesia arnolii*. The species has a large tuber 5 feet round, from which is pushed up a single leaf, with a long, stout petiole, the divided blade covering an area of 45 feet, or 15 metres.—Dr. R. C. A. Prior showed a specimen of *Colletia cruciata* in blossom, grown out of doors in Somersetshire by the Rev. W. Sotheby.—"Notes on Euphorbiaceæ," by Mr. G. Bentham, read in title, was a paper treating of the history, nomenclature, systematic arrangement, and the origin and geographical distribution of this remarkable order of plants. Among Dicotyledons, Euphorbiaceæ stands fourth in point of numbers, having above 3000 species and 200 genera. In investigating the origin of the order the geological record, unfortunately, is of no assistance. Their evident, generally tropical nature, is a striking feature, and, judging from various data, it is conjectured that their most ancient home was in the old world. Their affinities have repeatedly been discussed by botanists, but though there are individual genera which may exhibit some one character supposed to ally to other orders, yet no real connection has hitherto been pointed out. Their isolation is produced, not so much by any one special character, as by a special combination of several. As to position in the linear series, unless the order be broken up, practically it must remain among the Monochlamydeæ, in spite of occasional presence of corolla in some forms. The author has a most interesting chapter on nomenclature and synonymy, well worthy the study and serious attention of biologists generally. —Mr. Lewis A. Bernays, in a letter to the secretary, records the undoubted existence of *Carpesium cernuum*, in Queensland, and suggests its being indigenous there.—In a paper given in abstract, "Descriptions of New Hemiptera," by Dr. F. Buchanan White, the diagnosis of 2 new genera (*Helenus* and *Neovelia*) and 17 new species are entered. These mainly are the results of Prof. Trail's late exploration of the regions bordering the River Amazon.—Mr. Alfred W. Bennett read a communication, "Notes on Cleistogamic Flowers; chiefly of *Viola*, *Oxalis*, and *Impatiens*." According to him there are two kinds:—(1) Those which hardly differ from the perfect open flowers, other than the partial or entire suppression of the corolla, and the closing of the calyx (= homocleistogamic); and (2) those with a distinct modification in the flower to aid self-fertilization (= heterocleistogamic). He was at first disposed to regard those two kinds as having arisen, one by degradation, the other by a rudimentary form of the organ: but subsequent examination convinced him that both kinds owe their origin to degradation. In the extreme cleistogamic flowers a large number of organs have been correlatively modified. Most interesting phenomena occur in the mode of emission of the pollen tubes, these travelling through the air in a straight line from the anther vertically upwards in *Oxalis*, horizontally in others, and creep along the surface and even back of ovary in *Viola canina*. An unseen agency directs, for none wander with uncertainty; and this is all the more remarkable because, when not in proximity to the stigma, the pollen grains protrude their tubes in all possible directions.—The Rev. G. Henslow orally delivered the gist of a paper "On the Absorption of Dew and Rain by the Green Parts of Plants" (*vide Science Notes*).—The Rev. W. W. Fowler and Messrs. Wilfred Huddlestone and T. M. Shuttleworth were elected Fellows of the Society.

Chemical Society, November 7.—Dr. Gladstone, president, in the chair.—The following papers were read:—Contributions from the Laboratory of Tôkio, Japan. On the red colouring matter of the *Lithospermum erythrorhizon*, by M. Kuhara. The purple colouring matter was prepared from the root by extracting with alcohol, purifying by treatment with lead acetate, &c.; it forms a dark, resinous, uncrystallizable mass, with a metallic green reflection, soluble in alcohol, ether, benzol, almost insoluble in water; it resembles in some respects anchurin, the colouring matter from alkanet. A bromine and a chlorine compound were prepared.—A second report on some points in chemical dynamics, by C. R. A. Wright, and A. P. Luff. The authors have continued their previous research and have determined the temperature of initial action of carbonic oxide, hydrogen, and carbon on various oxides of iron, manganese, lead, cobalt, and nickel. They find that the general law holds good, that the temperature of the action of carbonic oxide lies below that of hydrogen, which again is below that of carbon; this rule appears to be a special case governed by the general law that *cæteris paribus* the greater algebraically is the heat evolution taking place during a reducing action on a metallic oxide, the lower is the temperature at which the action is first noticeable during a few minutes' action.—Note on the constitution of the olefine produced by the action of zinc upon ethylic iodide, by Dr. Frankland and Mr. Dobbin. The gas given off was passed through alcohol and sulphuric acid, and then absorbed by antimonic chloride; on heating with water and distilling a chloride was obtained, boiling at 83° C.; it was therefore ethylenic, and not ethylidenic chloride.—On the occurrence of certain nitrogen acids amongst the products of the combustion of coal gas and hydrogen flames, by L. T. Wright. The author proves that the origin of the nitrogen acids found in the condensed water procured by burning coal gas or hydrogen in the air is ammonia, either free or combined, no such acids being produced when the gases are carefully freed from ammonia.—On the action of bromine upon sulphur, by J. B. Hannay.—Researches on dyeing. Part I. Silk and rosanilin, by Dr. Mills and Mr. G. Thomson. The authors have investigated the nature of the transaction which occurs when a vat is exhausted of its tinctorial ingredients. The experiments consisted in immersing a constant area of white silk in a solution of a rosanilin salt at a constant temperature for varying times, and then determining the loss of strength of the rosanilin solution.—Comparison of the actions of hypochlorites and hypobromites on some nitrogen compounds, by H. J. H. Fenton. The compounds selected were, ammonium carbamate, guanidine, and biuret.—Notes on two new vegeto-alkaloids by

F. von Müller and L. Rummel. The authors have prepared alstonin from the bark of alstonia constricta, and duboisin from the leaves and twigs of duboisia myoporoides; the latter closely resembles piturin.—On the determination of lithia by phosphate of soda, by C. Rammelsberg. The author confirms his previous results as to the formation of a double salt of sodium and lithium phosphate and the consequent inaccuracy in lithia determinations made by Mayer's method; he also gives some analyses of lithia micas.

Physical Society, November 9.—Prof. G. C. Foster, vice-president, and afterwards Prof. W. G. Adams, president, in th- chair.—The following candidate was elected a Member of the Society:—Sir Frederick Elliot.—Prof. W. G. Adams explained a simple appliance made by Mr. S. C. Tisley for exhibiting the coloured bands due to interference with thick plates. The bands due to regular reflection and refraction were produced by two thick plates nearly parallel to each other and fixed in a brass box with rectangular apertures on its flat faces so that the light fell on the first plate at an angle of 60°, the whole apparatus being of a convenient size for the waistcoat pocket. On a previous occasion (June 23, 1877), Prof. Adams exhibited these bands to the Society, but not in a portable form. The elliptical interference bands, due to the scattering or diffusion of light at a point on the front surface of one of the plates, were shown by means of a precisely analogous arrangement, except that the inclination of the plates to each other was somewhat greater; in this case the interference bands, formed by *regular* reflection and refraction, fall in another direction, so that they are not received by the eye; the diffusion interference fringes obtained were clearly visible when thrown on the screen. They are formed by rays once diffused from points on the first surface and afterwards regularly reflected and refracted from the front and back faces of the two plates in succession. Prof. Adams pointed out that this instrument would form a convenient means of obtaining polarized light in cases where the length of a Nicol's prism is objectionable, for instance, under the stage of a microscope; the light will be completely polarized if the plates be placed to receive the light at the polarizing angle, and the field will be much brighter than when a plate of tourmaline is employed.—Prof. W. F. Barrett exhibited and explained Edison's microtasimeter and carbon telephone. These have been recently described in NATURE. In the course of a brief recapitulation of the history of these instruments, he referred to Du Moncel's early observations, published in 1856, that variations in the resistance of a circuit can be produced by varying the pressure on metallic surfaces in contact, and after referring to Clérac's plumbago rheostats he stated that Edison was probably the first to apply the diminished resistance of carbon under pressure to a practical use, which he did early in 1877 in his carbon relay, the progenitor of the carbon rheostat, micro-tasimeter and carbon telephone. In all he uses compressed lampblack, a button of which may be formed as follows. The wick of a paraffin lamp having been cut so that it smokes, a quantity of lampblack is formed in the chimney; the lower portion, which has the more intense black colour, is collected from time to time, and all brown particles must be carefully removed, since they offer a greater resistance. The mass is compressed into a disc about the size of a sixpence, crushed, passed through a fine sieve, and again compressed, and this operation may be two or three times repeated in order to attain to perfect uniformity. The original form of tasimeter, in which the hard rubber or other substance was placed horizontally, has been modified so that the whole is vertical. The carbon button rests on a smooth metallic surface in connection with a binding screw, and a similar conducting surface rests upon it leading to a second binding screw. A strip of hard rubber 1 inch long, $\frac{1}{4}$ inch wide, and $\frac{1}{16}$ inch thick, is supported vertically above it, its upper end being attached to a fine screw worked by a tangent screw with graduated head. The whole is inclosed in a heavy conical brass box. Prof. Barrett suggested that it would be preferable to make this jacket cylindrical, and that the whole should be inverted, because the weight of the strip on the button is found to prevent the needle of the galvanometer returning at once to zero. Employing one Daniell's cell and inserting a shunt, Wheatstone's bridge, and resistance coils in the circuit, it was shown that the hand, at some distance, caused a considerable deflection, and Prof. Barrett stated that in a still room the instrument becomes so sensitive as to be almost unmanageable. By replacing the hard rubber by a strip of gelatine varnished on one side, a very slight change in the hygrometric state of the atmosphere can be detected by the absorption of moisture causing expansion of the gelatine, and, therefore, compression of the carbon. Its action as an aneroid baroscope was suggested by Prof. Barrett, the button being associated with an exhausted box. He pointed out that before the tasimeter can be used as a measuring instrument, experiments must be made in order to ascertain the exact relation between the resistance of carbon and the pressure to which it is subjected. The carbon telephone, full particulars of which will be found in NATURE, vol. xix. p. 12, was next described, and Mr. Adams, Mr. Edison's assistant, now in England, exhibited a complete transmitting apparatus, with call, &c. A very ingenious and simple form of shunt, received from Mr. Edison with the tasimeter, deserves mention. A row of brass studs fixed on a board are united by plugs, so that if the current enters at one end it can pass out at the other without meeting with any appreciable resistance. But if a plug be removed it throws in about 4 inches of a resisting wire wound over two rows of pins, underneath the board, one row of which is in metallic connection with the studs; thus the entire length of wire is in circuit when all the plugs are removed. Finally, Prof. Barrett mentioned that a communication has just been received from Mr. Edison stating that he has succeeded in arranging an efficient receiving instrument in which no form of magnet is employed.—Mr. Ladd then showed several forms of electric lamp arranged so as to render the use of clockwork unnecessary. In that known as Wallace's workshop lamp the spark passes between the edges of two plates, the lower one being fixed while the upper one is raised to a suitable distance by an electro-magnet brought into action immediately on the passing of the current. A second form, in which an annular magnet was employed, acted on the same principle, the armature carrying the upper plate being specially arranged so as to give a maximum of attractive force. In the third form, the V-lamp, two rods of graphite were inclined at an angle of 45° to the vertical, resting in contact on a piece of china. Immediately on the current passing an electro-magnet is caused to act, and, after the rods have been firmly gripped, they are separated and the support removed. Should the circuit be broken they will at once fall together.

VIENNA

Imperial Academy of Sciences, October 10.—The deaths of Dr. Rokitansky and Prof. Tomaschek were referred to.—The following among other papers were read:—The dolomite ridges of South Tyrol and Venetia (Heft 2 and 3), by Dr. Mojsisovics.—On electric penetration of glass, by Prof. Mach.—On the relation of diffusion-phenomena to the second proposition of the mechanical theory of heat, by Prof. Boltzmann.—Calorimetric research on the heat of combination of carbonic acid gas and ammonia gas, to carbonate of ammonia, by Herr Lecher.—Action of radiant heat of the sun on a body in shade—time of occurrence of maximum temperature, by Herr Schlemüller.—Description of a telescope, by means of which, with one objective, you may point on two objects simultaneously, one distant, the other near, (sealed packet) by Herr Schneider.—Physical experiments, by Dr. Gross.—Distance reflector with precision-reading, by Herr Kuczera.—Discovery of a comet, by Mr. L. Swift.—Meteorite fall at Tieschitz in Mähren, July 15 last year, by Herr Tschermak.—Development-history of the prothallium of *Scolopendrium vulgare*, Sym., by Dr. Beck.

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